COK-HO6186-13-P16296

13

THE

BRITISH JOURNAL

OF

PSYCHOLOGY

CAMBRIDGE UNIVERSITY PRESS
C. F. CLAY, MANAGER
LONDON: FETTER LANE, E.C. 4

H. K. LEWIS & CO., LTD., 136, GOWER STREET, LONDON, W.C. 1
WHELDON & WESLEY, LTD., 2–4, ARTHUR ST., NEW OXFORD ST., LONDON, W.C. 2
CHICAGO: THE UNIVERSITY OF CHICAGO PRESS
(AGENT FOR THE UNITED STATES AND CANADA)
BOMBAY, CALCUTTA, MADRAS: MACMILLAN & CO., LTD.
TOKYO: THE MARUZEN-KABUSHIKI-KAISHA

All rights reserved

THE

BRITISH JOURNAL
OF
PSYCHOLOGY

GENERAL SECTION

EDITED BY

CHARLES S. MYERS

WITH THE ASSISTANCE OF

F. C. BARTLETT, S. S. BRIERLEY, AND C. BURT

AIDED IN THE SELECTION OF PAPERS BY

E. A. BOTT
W. BROWN
J. DEEVER
B. EDGELL
G. DAWES HICKS
W. McDOUGALL
T. H. PEAR
CARVETH READ
A. F. SHAND
C. S. SHERRINGTON
C. SPEARMAN
JAMES WARD
H. J. WATT
G. UDNY YULE

VOLUME XIII 1922—23

CAMBRIDGE
AT THE UNIVERSITY PRESS
1923

P16296

PRINTED IN GREAT BRITAIN

CONTENTS OF VOL. XIII

PART 1. JULY, 1922

PAGE

John Locke on the General Influence of Studies. By WILLIAM PHILLIPS 1

Recent Contributions to the 'Theory of Two Factors.' By C. SPEARMAN 26

Some Problems of Adolescence. By ERNEST JONES 31

A Vindication of the Resonance Hypothesis of Audition. (IV.) By C. R. G. COSENS and H. HARTRIDGE 48

Individual Differences in Listening to Music. By CHARLES S. MYERS 52

Age Standards for the Separate Northumberland Tests. (With Two Figures.) By GODFREY H. THOMSON 72

The Biological and Social Significance of the Expression of the Emotions. By CAMILLE NONY 76

Critical Notice of Cyril Burt's *Mental and Scholastic Tests*. By P. B. BALLARD 92

Publications Recently Received 96

Notes on Recent Periodicals 103

Proceedings of the British Psychological Society 106

British Psychological Society, Names of Officers and Members, 1922 *at end*

PART 2. OCTOBER, 1922

The Relations of Complex and Sentiment. (I.) By W. H. R. RIVERS 107

The Relations of Complex and Sentiment. (II.) By A. G. TANSLEY 113

The Relations of Complex and Sentiment. (III.) By ALEXANDER F. SHAND 123

The Relations of Complex and Sentiment. (IV.) By T. H. PEAR . 130

The Relations of Complex and Sentiment. (V.) By BERNARD HART 141

The Relations of Complex and Sentiment. (VI.) By CHARLES S. MYERS 146

A Note on Some Dreams of a Normal Person. By MARY STURT . 149

Motor Capacity with Special Reference to Vocational Guidance. By B. MUSCIO 157

A Vindication of the Resonance Hypothesis of Audition. (V.) (With Two Figures in Text.) By H. HARTRIDGE 185

Suspicion. By ALEXANDER F. SHAND 195

A Note on Local Fatigue in the Auditory System. By F. C BARTLETT and H. MARK 215

Critical Notice of H. Eliot Howard's *Territory in Bird Life*. By CARVETH READ 219

Publications Recently Received 223

Proceedings of the British Psychological Society 228

Part 3. January, 1923

PAGE
An Analysis of Literary Appreciation. By Olive A. Wheeler . 229
The Effects of Tensions of the Body Surface upon the Normal Dream. By A. J. Cubberley 243
Further Note on the 'Theory of Two Factors.' By C. Spearman . 266
Bilingualism and Mental Development. By Frank Smith . . 271
Hand and Ear Tests. (With Three Figures in Text.) By Hugh Gordon 283
Some Observations on Contrast Effects in Graded Discs. (With Six Figures and One Diagram in Text.) By Robert H. Thouless 301
The Interpretation and Plotting of Output Curves. (With One Figure in Text.) By Eric Farmer 308
The Constancy of the Intelligence Quotient. By P. L. Gray and R. E. Marsden 315
Publications Recently Received 325
Notes on Recent Periodicals 328
Proceedings of the British Psychological Society 336

Part 4. April, 1923

McDougall's Treatment of Experience. By Mary W. Calkins . 337
Vocational Tests and Typewriting. By B. Muscio and S. C. M. Sowton 344
The Transfer of Improvement in Reasoning in School-Children. By W. H Winch 370
Experiments on the Estimate of Duration. By Mary Sturt . . 382
An Application of American Army Intelligence Tests. By S. Bowie 389
The Influence of Subjective Preference on Memory. By Charles Fox 398
The Influence of Feeling on Memory. By A. Wohlgemuth . . 405
The Effects of Deprivation of Oxygen upon Mental Processes. By J. P. Lowson 417
A Preliminary Note on a New Method of Determining the Phase Effect in the Localization of Sound. (With Two Figures in Text.) By H. Banister 435
Publications Recently Received 437
Notes on Recent Periodicals 440
Proceedings of the British Psychological Society 443

LIST OF AUTHORS

PAGE

Ballard, P. B. Critical Notice of Cyril Burt's *Mental and Scholastic Tests* 92
Banister, H. A Preliminary Note on a New Method of Determining the Phase Effect in the Localization of Sound. (With Two Figures in Text) 435
Bartlett, F. C. and Mark, H. A Note on Local Fatigue in the Auditory System 215
Bowie, S. An Application of American Army Intelligence Tests . 389
Calkins, Mary W. McDougall's Treatment of Experience . . 337
Cosens, C. R. G. and Hartridge, H. A Vindication of the Resonance Hypothesis of Audition. (IV.) 48
Cubberley, A. J. The Effects of Tensions of the Body Surface upon the Normal Dream 243
Farmer, Eric. The Interpretation and Plotting of Output Curves. (With One Figure in Text) 308
Fox, Charles. The Influence of Subjective Preference on Memory 398
Gordon, Hugh. Hand and Ear Tests. (With Three Figures in Text) 283
Gray, P. L. and Marsden, R. E. The Constancy of the Intelligence Quotient 315
Hart, Bernard. The Relations of Complex and Sentiment. (V.) . 141
Hartridge, H. and Cosens, C. R. G. A Vindication of the Resonance Hypothesis of Audition. (IV.) 48
Hartridge, H. A Vindication of the Resonance Hypothesis of Audition. (V.) (With Two Figures in Text) 185
Jones, Ernest. Some Problems of Adolescence 31
Lowson, J. P. The Effects of Deprivation of Oxygen upon Mental Processes 417
Mark, H. and Bartlett, F. C. A Note on Local Fatigue in the Auditory System 215
Marsden, R. E. and Gray, P. L. The Constancy of the Intelligence Quotient 315
Muscio, B. Motor Capacity with Special Reference to Vocational Guidance 157
Muscio, B. and Sowton, S. C. M. Vocational Tests and Typewriting 344
Myers, Charles S. Individual Differences in Listening to Music . 52
Myers, Charles S. The Relations of Complex and Sentiment. (VI.) 146
Nony, Camille. The Biological and Social Significance of the Expression of the Emotions 76
Notes on Recent Periodicals 103, 328, 440
Pear, T. H. The Relations of Complex and Sentiment. (IV.) . 130
Phillips, William. John Locke on the General Influence of Studies 1
Proceedings of the British Psychological Society . 106, 228, 336, 443
Publications Recently Received 96, 223, 325, 437
Read, Carveth. Critical Notice of H. Eliot Howard's *Territory in Bird Life* 219
Rivers, W. H. R. The Relations of Complex and Sentiment. (I.) 107

PAGE
SHAND, ALEXANDER F. The Relations of Complex and Sentiment. (III.) 123
SHAND, ALEXANDER F. Suspicion 195
SMITH, FRANK. Bilingualism and Mental Development . . . 271
SOWTON, S. C. M. and MUSCIO, B. Vocational Tests and Typewriting 344
SPEARMAN, C. Recent Contributions to the 'Theory of Two Factors' 26
SPEARMAN, C. Further Note on the 'Theory of Two Factors' . 266
STURT, MARY. A Note on Some Dreams of a Normal Person . . 149
STURT, MARY. Experiments on the Estimate of Duration . . 382
TANSLEY, A. G. The Relations of Complex and Sentiment. (II.) . 113
THOMSON, GODFREY H. Age Standards for the Separate Northumberland Tests. (With Two Figures) 72
THOULESS, ROBERT H. Some Observations on Contrast Effects in Graded Discs. (With Six Figures and One Diagram in Text) . 301
WHEELER, OLIVE A. An Analysis of Literary Appreciation . . 229
WINCH, W. H. The Transfer of Improvement in Reasoning in School-Children 370
WOHLGEMUTH, A. The Influence of Feeling on Memory . . 405

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETINGS.

May 13, 1922 106
July 15, 1922 228
November 11, December 16, 1922 336
February 24, March 24, 1923 443

SECTIONAL MEETINGS.

(a) EDUCATION SECTION.

May 9, 1922. 106
October 10, November 7, December 5, 1922 336
January 1, February 13, March 13, 1923 443

(b) INDUSTRIAL SECTION.

July 15, 1922 228

(c) MEDICAL SECTION.

June 28, 1922 106
October 25, November 29, December 20, 1922 336
January 24, February 28, March 22, 1923 443

(d) AESTHETICS SECTION.

May 17, 1922 106
January 29, March 5, 1923 443

JOINT MEETING.

EDUCATION AND INDUSTRIAL SECTIONS.

June 13, 1922 106

VOLUME XIII JULY, 1922 PART 1

THE BRITISH
JOURNAL OF PSYCHOLOGY
GENERAL SECTION

JOHN LOCKE ON THE GENERAL INFLUENCE OF STUDIES.

BY WILLIAM PHILLIPS.

A. *Locke's views on the influence of specific studies compared with modern opinion as set forth in five Positions.*
Position 1. *Formal training and the education of an expert* (pp. 2–4).
Position 2. (*a*) *All training tends to produce general effects.* (*b*) *The uncertainty of the direction this tendency will take* (pp. 4–9).
Position 3. (*a*) *The search for a principle by which to determine whether a body of knowledge is absolutely valid or not has so far failed.* (*b*) *The search for a method that will be equally applicable to every inquiry has also failed* (pp. 9–14).
Position 4. *The generalising effects of a study largely dependent on the time for which it is pursued* (pp. 14, 15).
Position 5. *The possibility of securing any training whatsoever dependent on the congenital endowment* (pp. 15–17).
B. *Did Locke place the emphasis, not on the subject studied or the act of skill practised, but on the process of learning? Is his system of moral training entirely disciplinary in character?* (pp. 17–23).
Conclusions (pp. 23–25).

A.

VARIOUS attempts have been made to arrive at Locke's views on the general effects produced on mind and body by certain studies and exercises. It cannot be said that the conclusions arrived at by the various inquirers are in any way unanimous. The present article is another effort to establish his views on this matter, and to arrive at his opinions on various aspects of Formal Training. I propose to lay down certain Positions which appear to me to be representative of the best opinion of to-day; some of them will give a measure of support to a theory of Mental Discipline, while others will indicate that a considerable abatement must be made in the claims often put forward in favour of basing educational practice on such a theory. I propose to examine Locke's works in order to discover which of these Positions were occupied by him.

Position 1. *Every bodily and mental act must be practised by itself if it is to be performed well, and expertness in the performance of any operation can only be got through special training. An examination of the kind of education required to produce an expert shows that a merely Formal Training is entirely insufficient.*

So far as I have been able to discover, Locke nowhere discusses the question of educating an expert. In regard to manual operations, he takes quite a modern view in respect to this First Position, and one which may be recommended to the consideration of certain advocates of so-called Manual Training. "Would you," he says, "have a man write or paint, dance or fence well, or perform any other manual operation dexterously and with ease, let him have ever so much vigour and activity, suppleness and address naturally, yet nobody expects this from him unless he has been used to it, and has employed time and pain in fashioning and forming his hand or outward parts *to these motions*" (my italics)[1]. He was quite alive to the fact that certain acts of skill, such as those involved in Music and Painting, demand a long course of training, and that at its close, some persons attain only an imperfect command over them: see the *Thoughts*, § 197 on Music, and § 203 on Painting. Nevertheless, he certainly underestimates the time and practice required in order to gain even a fair command over one manual art, for he says that a youth may find time enough "to acquire *Dexterity and Skill in hundreds of Things*" (his own italics)[2].

With regard to mental studies, his view is not fully expressed in the oft-quoted § 94 of the *Thoughts*, where he says that the Studies which a Governor sets his Pupil upon are but as it were the Exercises of his Faculties and Employment of his Time. In § III of the *Conduct*, he looks at the matter from another point of view and refers to the necessity for cultivating a wide acquaintance with every subject a man wishes to deal with. "The faculty of reasoning," he says, "seldom or never deceives those who trust to it; its consequences from what it builds on are evident and certain, but that which it oftenest, if not only, misleads us in is that the principles from which we conclude, the grounds upon which we bottom our reasoning, are but a part, something is left out which should go into the reckoning to make it just and exact"[1]. Here he seems for the moment to be concerned with the necessity for making an intensive study of the subject or subjects we propose to pass judgment upon, but the close of the section shows that he is all the time more impressed by the help given to good reasoning by wide rather than by specialised knowledge. "The want of exercising [the reason] in the full extent of things intelligible, is that which weakens and extinguishes this noble faculty in us"[2]. In § 186 of the *Thoughts* he does however approach to a recommendation of what may be called a specialised study, for the "*General Part of Civil-Law* and History are studies which a Gentleman should not barely [merely] touch at, but constantly dwell upon, and never have done with." This, it will be noticed, is a suggestion for adult study.

---

[1] *Conduct*, § VI, pp. 19–20: my references to pages are to Fowler's ed. In quoting from the *Thoughts*, I use Quick's ed., and that of Fraser for the *Essay*. Refs. to Locke's *Works* are to the ten vol. ed., 1823.

[2] *Thoughts*, § 208, p. 182.

This neglect of the claims of specialisation in any part of a youth's education is largely accounted for by the still backward state of many subjects in Locke's day: mathematics was indeed already a formidable science, but chemistry, physics, botany, geology, political science, as well as many others, were represented by very small collections of facts and theories. As Adamson points out, "the 'new philosophy' was not yet sufficiently advanced, elaborated and systematized to be made an agent of education. Sprat, in his *History of the Royal Society* (1667), deprecated the notion that the new body would encroach upon the work of the universities, seeing that its studies were unsuited to undergraduates"[3].

How wanting in scientific precision, method and technique some important subjects were in Locke's time is well illustrated by the state of medicine, and by the apparently haphazard manner in which he himself became a doctor. Fox Bourne, Fraser and others make this point clear[4].

It is safe to say that Locke had only a weak conception of the long and arduous special studies that are necessary for the making of an expert. In his day, there was a good deal of justification for the notion that a pupil who had been put by his tutor in the right way of knowing and improving himself could, when he had a mind to it, master the advanced parts of most subjects then known[5]. But modern educators cannot fail to realise the extremely specialised character of the data and methods employed in so many branches of knowledge to-day, and therefore cannot help abating the claims made by Locke on behalf of the formal aid given to scientific inquiry by general study.

---

[1] *Conduct*, § III, p. 7.

[2] *Conduct*, § III, p. 10. Cf. § VII, p. 24; § XII, p. 35; § XIX, p. 44.

[3] *The Educational Writings of John Locke*, ed. by J. W. Adamson (1912), p. 1.

[4] *Life of John Locke*, H. R. Fox Bourne (1876), I. 128; *Locke*, A. C. Fraser (1907), p. 19.

[5] See *Thoughts*, § 195.

Position 2 (a). *But although each bodily and mental operation has to be practised by itself, and even then it is often difficult to secure as much as a fairly correct performance of it, nevertheless the effect of the practice is not confined to that one operation, for it may influence another, different in some respects from it.* Evidence is available to prove, that one bodily operation influences a second, even in cases where it is unlikely that either the unconscious or conscious mind takes a controlling part in it. (As an instance of this, Davis's researches into the effect of movements performed by one arm or hand on those done by the other arm and hand may be mentioned. See his "Researches in Cross Education": *Yale Psychological Studies*, 1898.) The evidence in favour of the view that this can take place, may be for good, may be for evil, under (1) the unconscious and (2) the conscious direction of the mind is still clearer. (For an instance of the first, note how a child tends to hold a painting brush in the same way as a pen-holder, or *vice versa*, according to the instrument he has learnt to manipulate first. For an instance of the second, showing how theory may influence practice, see Judd's account of the experiment he conducted on aiming at a target lying under water, as given in *Psychology of High School Subjects*, 1915, p. 268.) That the effect on the mind of doing one problem or meeting one situation, or doing or meeting one type of problem or situation is, either to the learner's advantage or disadvantage, not entirely specific, is still more obvious. (One of the most striking and unexpected instances of this is given by Fracker, who, in his researches into the "Transference of Training in Memory," found that a system of Imagery, called into play in dealing, under difficult conditions, with memorising the order in which four different tones were given, was by some of his subjects successfully transferred and used in recalling quite a different material, such as the order in which grays were presented. Whether these investigations of Fracker yield any pedagogical guidance or not, they are undoubtedly of high psychological interest. See *Psychological Review Monograph Supplements*, vol. IX, No. 2.) The mind tends to treat each situation in a general way.

Locke was of course ignorant of any such detailed psychological methods as are hinted at here: furthermore he had no place in his philosophy for the subconscious. On the other hand, he was fully aware of the fact that the mind acts in a complex way: in one place he speaks of the legerdemain of the mind[1]. But those who have studied his psychology know that it forms no clear and consistent scheme. At one time, he speaks as if the mind were entirely passive in its reception of ideas, at least, that it never alters or modifies them: at another time, he tells us that "sometimes the mind fixes itself with so much earnestness on the contemplation of some objects, that it turns their ideas on all sides; remarks their relations and circumstances; and views every part so nicely and with such intention, that it shuts out all other thoughts, and takes no notice of the ordinary impressions made then on the senses"[2], and again in speaking of 'general and universal,' he states that "they belong not to the real existence of things; but are the inventions and creatures of the understanding, made by it for its own use"[3]. Indeed, as distinguished from our simple ideas, all other ideas are spoken of by him as being in some sense 'the work of the mind'[4]. But this work is mainly that of compounding and enlarging: Locke has no insight into the process of Apperception. I do not mean that he does not discuss various mental processes and products which we include under the category of apperception: what is meant is that his view of their character is imperfect and largely based on mechanical analogies. His view therefore of the nature of Universals is, as Gibson clearly points out, inadequate. "In accordance with the presuppositions of the composition theory, which Locke never formally withdrew or repudiated, the ultimate constituents of our knowledge, as yielded by experience, are represented as so many units, each simple and complete in itself. But unless the universal is in some manner present in the most elementary form of cognitive experience, it can never be elicited from it by any processes of abstraction and 'consideration.' And unless the single idea is from the first implicitly apprehended as belonging to a larger whole, no amount of manipulation by the mind can discover an intrinsic connection between it and any other idea"[5][6].

Locke is thus seen to provide no adequate explanation, for example, for such mental operations as those described by Stout under the heading "Relative Suggestion"[7]. It is true he discusses Analogy in the *Essay*

---

[1] *Conduct*, § XLI, p. 89.

[2] *Essay*, II. 19. 3, F. 1. 299–300.

[3] *Op. cit.* III. 3. 11, F. 2. 21.

[4] Cf. *Locke's Theory of Knowledge and its Historical Relations*, James Gibson (1917), p. 61.

[[5] It is a forgetfulness of this fact that invalidates much of Thorndike's discussion of Formal Training in his *Principles of Teaching* (1906): in particular p. 248.]

[6] Gibson, *op. cit.* p. 322.

[7] *Anal. Psychology*, G. F. Stout, Chap. VI.

(IV. 16. 12, F. 2, 379), and the *Conduct* (§ XL, p. 86), but his psychology cannot account for it. This point will however come up later.

Position 2 (b). *The fact that certain situations contain one or more recurring factors makes it likely that the ordinary mind will notice them and treat the situations in which they occur in a uniform or approximately uniform way. It is also possible sometimes to isolate these factors and consciously compare and contrast them, and so formulate Methods or Rules for dealing with not only the particular but also the general situations to which they belong.* (This is the task to which the teacher has to bend his main energies, and he knows how difficult it is to secure a small degree of mastery over it from many of his pupils. The introduction of a novel element into an old situation is often sufficient to gravel them in their attempts at applying their already acquired methods or rules to it.) Because of this, we sometimes speak of objective identities and similarities, and distinguish them from subjective ones, but this is a mere convention. It is found convenient to reserve the latter term for those factors whose influence on the mind is of a less predictable character, and which depends on some peculiarity of constitution, either hereditary or acquired, shown by the individual mind affected by it. (The instance quoted above of the use of Imagery by Fracker's subjects illustrates this. Sleight found that practice in learning prose extracts exercised no influence on the learning of nonsense-syllables, but the memorising of poetry and arithmetical tables did have a powerful influence: in the latter case, rhythm exercised an unexpected effect[1].) Positions 2 (*a*) and (*b*) indicate that the theory of Formal Training is not without some basis in a true psychology of the mind.

I have stated that Locke was less affected by the extreme views that are contained under the phrase 'Manual Training' than are many modern educationists. It is true he does not explicitly discuss the point. We have his views on Instrumental Music in the *Thoughts*, § 197, on Fencing in § 199, and on acquiring dexterity in a Trade in § 201, etc.: his remarks on Dancing in § 196 are also worth looking at. In § 208, he certainly underestimates the time and practice required to gain mastery over even one manual art. In the *Conduct* (§ VI, pp. 18–20), he discusses some of the obstacles that stand in the way of securing a proper employment of the mind in reasoning, but he does not, as so many do, compare this to what happens in hand-training in general, but rather to particular arts like engraving, designing or writing. Later on in the same section occurs the passage already quoted above, in which he shows that even the most dexterous person must undergo a special training in writing if he wishes to write well, in painting if he wishes to become a skilful painter, and so on: a man's hand must be specially formed to perform special 'motions.' There is no ground for believing that Locke would have subscribed to some of the claims put forward on behalf of a training in Sloyd, such as those detailed in Salomon's *Theory of Educational Sloyd*[1].

---

[1] "Memory and Formal Training," W. G. Sleight, *Brit. Journ. of Psych.* IV. (1911), pp. 423, 437.

He also believes that certain mental acts must be specially practised, and furthermore that no general effect is thereby produced. In regard to Memory, for instance, and in a less degree to Attention, he seems expressly to deny that the act of memorising or attending to one datum or set of data can facilitate the memorising of or attending to other data[2]. It must however be noted that he does not give us his opinion on the question whether the learning of one group of facts improves the memory for other facts belonging to the same group. "What the Mind is intent upon," he adds, "and careful of, that it remembers best: To which, if Method and Order be joined, all is done, I think, that can be, for the Help of a weak Memory"[3]. It is doubtful whether Locke's psychology provides any room for the operation of Method and Order. The fact however that he refers to them at all shows he in a vague way felt that the presence of some general factor had to be accounted for in the increased ease and accuracy with which some people come to memorise certain classes of data systematically dealt with by them.

In his treatment of Attention, Locke deals with two points that are given an even more prominent place in his account of Memory. The first is that Attention largely depends on a person's natural constitution, and the second that if the mind is to be employed upon a particular set of facts, it must be practised in dealing with it. "The end and use of a little insight," he says, "in those parts of knowledge, which are not a man's proper business, is to accustom our minds to all sorts of ideas and the proper ways of examining their habitudes and relations"[4]. (We know however that he greatly fears the effect of attending to only one or two subjects, for "let a man be given up to the contemplation of one sort of knowledge, and that will become everything"[5].) The mind develops a propensity to turn its attention to what it has long been occupied with and keeps it there for a considerable period easily, and derives much pleasure thereby. This effective kind of attention is, Locke well knows, of a slow growth. The mind brings itself to attention and close thinking by insensible degrees[1]. He does, it is true, even speak of getting the habit of attention and application[2], but when one begins to examine the manner in which such a habit works in a particular study, one finds that what Locke prescribes is the cultivation of ways of attention suitable to that study itself and to a steady manipulation of it.

---

[1] See, in particular, p. 6, of that book.

[2] *Thoughts*, § 176, p. 154.

[3] *Op. cit.* pp. 154, 155.

[4] *Conduct*, § xix, p. 43.

[5] *Ibid.*

He is impressed by the "constant succession and flux of ideas in our minds"[3], and thinks that "it may be of great advantage, if we can by use get that power over our minds as to be able to direct that train of ideas"[4]. But he has no panacea for the ill. It is not so easy, he thinks, to cure this mind-wandering as perhaps some people imagine, but he does not rule the possibility of it out of court[5]. Elsewhere he says: "A lasting continued Attention is one of the hardest tasks can be imposed on them [children]: and therefore, he that requires their Application, should endeavour to make what he proposes as grateful and agreeable as possible"[6].

It is therefore I think fairly obvious that we possess no satisfactory evidence to justify us in asserting that Locke advocated any course of training for the purpose of cultivating a capacity of attention which would show itself in the study of any and every problem. He sees however that the long and successful prosecution of a study does, on the one hand, enable a person to attend effectively to it, but it may, on the other hand, make him indisposed to take up another, or may dispose him to deal with it on the lines of the old, even when they are unsuitable for that purpose. "Some men have so used their heads to mathematical figures that, giving a preference to the methods of that science, they introduce lines and diagrams into their study of divinity or politic enquiries"[7]. Although therefore Locke has no proper psychological theory to account for the fact, he dimly perceives that an act of attention to a particular situation produces something other than a specific effect on the mind. From the pedagogical point of view it is very important to note how clear his realisation is of the necessity for long and continued attention to the same data if easy and correct manipulation of them is to be obtained.

There is however one science for which Locke undoubtedly claims a beneficial influence of a general kind over the mind, namely, Mathematics. But it will be necessary in dealing with his treatment of this subject to keep in mind not only Positions 2 (*a*) and (*b*), but also Position 3.

---

[1] *Conduct*, § XXVIII, p. 63.

[2] *Op. cit.* § XXX, p. 67.

[3] *Op. cit.* § XXX, p. 66.

[4] *Op. cit.* § XXX, p. 67.

[5] *Ibid.*: cf. § XLV, p. 96.

[6] *Thoughts*, § 167, p. 142. The *Essay* throws little light on Locke's view of Attention.

[7] *Conduct*, § XXIV, pp. 50–51.

Position 3 (a). *The search for certainty in knowledge has not led to the discovery of unshakeable foundations for any of our sciences: modern investigation rather tends to show that even those departments, such as some branches of mathematics and physics, which were supposed to be inevitably true and valid, cannot now be credited with an absolute certainty.*

Locke was deeply interested in the search for the grounds upon which unfailingly true knowledge could be built. Only that knowledge which consists of certain and universal propositions appeared to him to be wholly satisfactory, and it is upon such universal and necessary propositions, he thinks, that the possibility of 'demonstration' depends, without which our knowledge would be without system, and could not assume the form of science. Locke, like Descartes and Spinoza, found the pattern of such knowledge in mathematics, and especially in geometry. "The perfect intellectual transparency, which appeared to him [Locke] to be possessed both by the primary assumptions of these sciences and by their demonstrations, constituted his ideal of knowledge, and formed the standard by which he tested the truth of all our intellectual possessions"[1]. Not only therefore was he convinced that mathematics presented an eternally valid type of knowledge, but he also believed that the methods of proof adopted in its various branches, especially in geometry, were ideal, and he showed a strong disposition to recommend the application of them in all other departments.

Position 3 (b). *The greater the growth of the various branches of knowledge and the more complicated their ramifications become, the clearer does it appear that there is no one method that is universally applicable in dealing with them all.* It is true that certain methods have been found to possess a very wide applicability; for example, the evolutionary principle in the study of every kind of living organism and the societies to which they belong, and several mathematical methods in political and social problems, where quantities of various kinds are considered. But more than this, we cannot say.

As has been said, spatial and numerical ideas seemed to Locke to possess a delightful and satisfactory transparency. In them 'demonstration' could be sought for and found. But why only in the mathematical sciences? "The reason," he says, "why it has been generally sought for, and supposed to be only in those, I imagine, has been, not only the general usefulness of those sciences; but because, in comparing their equality or excess, the modes of numbers have every the least difference very clear and perceivable: and though in extension every the least excess is not so perceptible, yet the mind has found out ways to examine, and discover demonstratively, the just equality of two angles, or extensions, or figures: and both these, *i.e.* number and figures, can be set down by visible and lasting marks, wherein the ideas under consideration are perfectly determined, which for the most part they are not, where they are marked only by names and words"[1].

---

[1] Gibson, *op. cit.* pp. 4, 5.

One aspect of Mathematics—the sciences (Locke refers to them in the plural), possessing "the greatest certainty, clearness, and evidence"—that made a strong appeal to him was that "from very plain and easy beginnings, by gentle degrees, and a continued chain of reasonings, [mathematicians] proceed to the discovery and demonstration of truths that appear at first sight beyond human capacity. The art of finding proofs, and the admirable methods they have invented for the singling out and laying in order those intermediate ideas that demonstratively show the equality or inequality of unapplicable quantities, is that which has carried them so far, and produced such wonderful and unexpected discoveries"[2]. Later on in this twelfth chapter on "The Improvement of our Knowledge," he discusses the question "whether natural philosophy be capable of certainty or no," and lays down two ways of enlarging our knowledge, the second being "the art of finding out those intermediate ideas[3], which may show us the agreement or repugnancy of other ideas, which cannot be immediately compared"; he states that "these two (and not the relying on maxims, and drawing consequences from some general propositions) are the right methods of improving our knowledge in the ideas of other modes besides those of quantity, the consideration of mathematical knowledge will easily inform us"[4]. He gives the following heading to the seventh section of this same chapter: "The true Method of advancing Knowledge is, by considering our abstract ideas": the steps by which we are to proceed in the attempt

[1] *Essay*, iv. 2. 10, F. 2. 183. (For a very helpful commentary on this passage, see Gibson, *op. cit.* p. 152). Cf. iv. 3. 18 and 20, iv. 17. 11, etc. on the value of Algebra: also *Conduct*, § vii, p. 24.

[2] *Essay*, iv. 12. 7, F. 2. 346–7.

[[3] For a geometrical instance, see *Essay*, iv. 2. 2, F. 2. 178–9.]

[4] *Op. cit.* iv. 12. 14 and 15, F. 2. 354, 355.

at discovering the relations that exist between these abstract ideas are to be learnt in the school of the mathematicians[1].

The Science of Mathematics was therefore to Locke the highest type of demonstrable and certain knowledge. He does however at times, with more or less assurance, put forward the view that Morality is also a science capable of "demonstration"[2]. But in the chapter of the *Essay* dealing with "The Improvement of our Knowledge," he does not speak on this point with full confidence. After pointing out the reasons why mathematicians have "produced such wonderful and unexpected discoveries," he adds: "But whether something like this, in respect of other ideas, as well as those of magnitude, may not in time be found out, I will not determine. This, I think, I may say, that if other ideas that are the real as well as nominal essences of their species, were pursued in the way familiar to mathematicians, they would carry our thoughts further, and with greater evidence and clearness than possibly we are apt to imagine"[3].

Spinoza, as we know, believed he had discovered a demonstrable system of ethics. Locke thought of making one too, but he never got far with the task. He was many times and by different persons pressed to attempt it. I cannot tell the whole story here, and so must refer my readers to Locke's *Works*, Vol. IX. 291, 294, 295, 299, 306, 310, 312–5, 328, 329, 332, 337, 347, 351–2, 374, 375, and 377. But such a work was never written by him.

This failure or indisposition to make the attempt at producing a demonstrable scheme of Ethics did not shake Locke's belief in Mathematics as the science in which full certainty could be obtained. He also believed that it was a body of knowledge of the greatest practical value, and furthermore, that a study of its arguments and methods supplied a considerable help in the study of other subjects. I have already referred above to those of its characteristics which in Locke's opinion make it so markedly helpful in dealing with general and abstract truths.

In his chapter on Reason, he makes still clearer what seem to him the exact virtues possessed by mathematics. He says that Reason may be considered as having *four degrees*: "the first (and highest) is the discovering and finding out of truths; the second, the regular and methodical disposition of them, and laying them in a clear and fit order, to make their connexion and force be plainly and easily perceived; the third is the perceiving their connexion; and the fourth, a making a right conclusion"[1]. He adds that these several degrees may be observed in any mathematical demonstration.

---

[1] *Op. cit.* IV. 12. 7, F. 2. 346.

[2] *Op. cit.* III. 11. 16, F. 2. 156; also IV. 3. 18, F. 2. 208; IV. 4. 7, F. 2. 232: cf. IV. 2. 9, F. 2. 182.

[3] *Op. cit.* IV. 12. 7 and 8, F. 2. 346–7.

Fowler very properly remarks that "the relation of the treatise on the Conduct of the Understanding to the Essay is that of a sort of practical appendix. The one book enquires into the constitution and history of the Human Mind, the other attempts to suggest rules and cautions for guiding or controlling its operations in the search for knowledge"[2]. What then are the practical applications (if any) which Locke makes of these four degrees of reason when he comes to advise us how to "conduct the understanding right in the search of knowledge and in the judgments it makes"?

A matter which he repeatedly emphasises in the *Conduct* is that "the faculties of our souls are improved and made useful to us just after the same manner as our bodies are"[3], that is, by practice. And so, "would you have a man reason well, you must use him to it betimes, exercise his mind in observing the connection of ideas and following them in train. Nothing does this better than mathematics"[4]. It will be noted that the only two degrees of Reasoning here explicitly referred to are the second and third. They are the same two aspects of mathematical reasoning that he comments on in the well-known Seventh Section of the *Conduct*, headed 'Mathematics.' He thinks that men having got the way of reasoning, "which that study necessarily brings the mind to, they might be able to transfer it to other parts of knowledge as they shall have occasion." Furthermore, he believes that the study of this science is of infinite use even to grown men[5], and for this he adduces three reasons. But the first 'degree' of reasoning is not discussed here at all. The omission in the *Conduct*, a work commenced seven years after the first appearance of the *Essay*, of any claim on behalf of mathematical study that it can effect a 'transfer' in the way of enabling a man to discover in other matters those intermediate ideas by which a 'demonstration' must be made, is very significant.

It is however essential to note that Locke does claim that application to mathematics, especially to geometry, tends to settle in the mind a *habit* of examining premisses and arranging arguments in train, and that once we have arrived at an improved way of reasoning, which mathematics brings the mind to, we shall be able to 'transfer' it to other parts of knowledge as we shall have occasion. This makes it obvious that Locke was, as has been said above, a believer in some forms at least of what we call Formal Training. (I do not know that he uses this term[1].) It was easier for him to believe in this transferable effect of mathematical study than for us, since he considered that "in all sorts of reasoning every single argument should be managed as a mathematical demonstration"[2]. Nevertheless he never attempts to prove this, and nowhere gives an illustration of it.

---

[1] *Essay*, IV. 17. 3, F. 2. 388.

[2] See his ed. of *Conduct*, p. xxiii. I may remind my readers that the *Essay* appeared in 1690; the *Thoughts* were written between 1683 and 1689, and appeared in 1693; the *Conduct* was begun about April 1697, but not published till 1706.

[3] *Conduct*, § VI, p. 19.

[4] *Op. cit.* § VI, p. 20.

[5] *Op. cit.* § VII, pp. 24–5.

The fact seems to be that he was favourably disposed towards mathematics, and looked upon it in quite a different mood to the one in which he viewed logic. In the case of the latter, his early attempts at mastering it were accompanied by painful experiences, which are absent from all his recorded recollections of his study of mathematics. It is possible geometry and algebra did not form a part of his work at Westminster School[3]. But whatever may have been the cause, he did not submit the use of mathematical methods in general subjects to the same critical examination as the syllogism received at his hands. Syllogism, he says, "serves our reason but in one only of the forementioned parts of it," that is, in the third degree of reason. Reason, he explains, contains two parts, Sagacity and Illation. But syllogism in no degree strengthens Sagacity, and even in Illation, "it is of no great use, since the mind can perceive such connexion, where it really is, as easily, nay, perhaps better without it"[4]. It is somewhat strange that having gone as far as this, he did not realise that any attempt at reducing our thoughts to the form adopted by Euclid in his geometrical demonstrations would also often be a clog on our reasoning in many directions.

But, as already indicated, his recollections of his early struggles with logical abstractions, were unpleasant. It is very likely that what he says in the *Thoughts*, §§ 188, 189[5], reflects his own experiences at Westminster and Oxford, and also indicates his subsequent opinion of the value of logical study. But in this he is however a little inconstant. Compare what he says in the letter to Sir John Banks (28 Aug. 1677)[6], with § 188, of the *Thoughts*, and also the statement in his "Second Vindication of the Reasonableness of Christianity" (1697), that the syllogism is "the true touchstone of right arguing"[1], with his general views as expressed in the *Essay*, where he will as a rule hardly admit that "mode and figure" are even of value in the stripping and detection of fallacies. On the other hand, he readily recognised the practical value of mathematics, the study of which, probably not commenced in school-days, had given him a large measure of satisfaction, and so had disposed him to make considerable claims on its behalf as a specially valuable form of mental exercise.

---

[1] He uses the phrase, *ad capiendum ingenii cultum*, in *Thoughts*, § 147, p. 129.

[2] *Conduct*, § VII, p. 23.

[3] Cf. *Annals of Westminster School* (1898), Sargeaunt, pp. 120–2.

[4] *Essay*, IV. 17. 4, F. 2. 389.

[5] See also, in particular, *Conduct*, § 1, p. 4. For his Oxford experiences, see Fox Bourne, *op. cit.* I. 47–8.

[6] Fox Bourne, *op. cit.* I. 378.

Position 4. *The length of time during which a person is engaged in learning to perform any mental or bodily processes, in repeating and applying them, and the extent to which mastery is obtained over complex combinations of them, largely determine whether the person so exercised will tend to perform other operations on analogous lines; also this may sometimes be an advantageous and at other times a disadvantageous procedure. This tendency can be guided into more accurate directions by a prosecution of other well-selected activities and studies.*

In the important Seventh Section of the *Conduct*, Locke concludes that it would be well if men's minds were accustomed to looking at questions from various sides, and that "early, that they might not erect their opinions upon one single view....This would enlarge their minds, and give a due freedom to their understandings.... To this perhaps it will be objected, that to manage the understanding, as I propose, would require every man to be a scholar, and to be furnished with all the materials of knowledge, and exercised in all the ways of reasoning." He practically admits that any such supposed objection would have much to be said for it. But "it is a shame," he says, "for those that have time and the means to attain knowledge, to want any helps or assistance for the improvement of their understandings that are to be got, and to such I would be thought here chiefly to speak." He therefore recognises that the mind must be exercised closely and for a long time in mathematical study before it is likely to apply the methods therein employed to reasonings on other matters. He clearly understands also that a long application to only one or a few studies may be harmful. He illustrates this by one of his rare jokes. "A metaphysician," he suggests, "will bring plowing and gardening immediately to abstract notions." "If men," he adds, "are for a long time accustomed only to one sort or method of thoughts, their minds grow stiff in it"[2].

The question that has more than once been asked whether there is an inconsistency between the special value placed by Locke on mathematics in the *Essay* and *Conduct*, and the merely utilitarian place assigned to Arithmetic and Geometry in the *Thoughts* may, I think, now be answered. In the latter he says: "A Man cannot have too much of Arithmetic, nor too perfectly"[1]: in Geometry, he thinks the first six Books of Euclid enough for the pupil to be taught, and he is "in some doubt whether more to a Man of Business be necessary or useful"[2]. But from this amount of mathematical study, Locke nowhere suggests he expects that those important and wide results which he thinks are obtainable from a long and detailed contemplation of mathematics, and spoken of by him in the *Essay* and *Conduct*, will follow.

---

[1] *Works*, VII. 385.

[2] *Conduct*, § XIX, pp. 43, 44.

Position 5. *The difficulty of teaching some individuals to perform certain acts of skill and certain classes of mental work is very considerable, and even when they have acquired a fair mastery over several such groups, their capacity for applying them in novel situations is limited. In fine, it is now agreed that the capacity for receiving any training at all, and the power of adapting it, when received, to meet new demands, vary greatly in different persons, and depend in the first place on their congenital constitutions, bodily and mental.* In some ways which have not yet been fully defined, a person's grade of memory, attention and intelligence is fixed at birth and can never be altered, at least to any marked extent. But the fact that some people, after pursuing hard courses of study, like mathematics, Latin, or logic (modern methods have of course removed much of their difficulty), were in many countries and periods observed to meet intricate situations and to perform complicated problems successfully, gave rise to the idea that the general ability and acute reasoning power therein shown were produced by the courses named, although they had little or no connection with the tasks performed.

I presume that many would, on reading this, be prepared to affirm that Locke's views are out of harmony with several parts of it. They would call attention to his dictum that the young child is like white paper or wax, to be moulded and fashioned as one pleases[3]. They would however do well to remember that the section in which this appears refers to "the various Tempers, different Inclinations, and particular Defaults, that are to be found in Children.... Each Man's Mind has some Peculiarity, as well as his Face, that distinguishes him from all others." It is therefore plain that Locke did not, by the use of this simile[4] of White Paper, intend to teach that we are all born equal in intellectual capacity. The words just quoted disprove such a supposition. Locke emphasises the disparity that exists between one mind and another in many places; for example, in the *Thoughts*, § 1, p. 1; § 66, pp. 39, 40; §§ 100, 101, 102, pp. 82, 83; § 161, p. 137; § 176, pp. 153, 154; § 203, p. 178; in the *Conduct*, § II, p. 5; § XXXVIII, p. 82; in the *Essay*, IV. 20. 5 (F. 2. 445–6), where it is particularly noteworthy that in discussing the causes why some are lacking in skill to use proofs, he thinks it may be due to the dulness of their faculties through want of use, "or, *as some think* [my italics], in the natural differences of men's souls themselves": he will not take the responsibility for the latter supposition, although he is prepared to say that "there is a difference of degrees in men's understandings."

---

[1] *Thoughts*, § 180, p. 157. [2] *Op. cit.* § 181, p. 158. [3] *Op. cit.* § 217, p. 187.

[4] "Similes always fail in some part, and come short of that exactness which our conceptions should have to things, if we would think aright"; *Conduct*, § xxxii, p. 72.

One remark made in the *Thoughts* is to be always borne in mind. "God has stamp'd certain Characters upon Men's Minds, which like their Shapes, may perhaps be a little mended, but can hardly be totally alter'd and transform'd into the contrary" (§ 66, p. 40). The comparison of the mind with white paper must, if pressed, be given up in face of this declaration that some pages have already been written on.

What then was his main object in making the comparison? It is, I think, clearly stated in the *Essay*, II. 1. 2 (F. 1. 121), where he says: "Let us then suppose the mind to be, as we say, white paper, void of all characters, without any ideas." That is the point he wants to emphasise first of all: man is not born in possession of any innate ideas or principles: he is born with a capacity for forming ideas, but he does not possess even one in a complete form: in that respect his mind is, like white paper, void. But there is a second point he wishes to impress upon us: the child's mind is open to receive practically any kind of impression we like to make upon it. Take the parenthetic remark contained in the following: "For such, who are careful...to principle children well,...instil into the unwary, and as yet unprejudiced, understanding (for white paper receives any characters), those doctrines they would have them retain and profess"[1]. The simile is one of many indications that Locke considered the effects of Education, Exercise and Practice, incalculably great.

It has however been thought that he meant by the use of this simile to imply that we are born equal in mental ability. I do not know of any passage in which he asserts this, and have quoted and referred to quite a number in which he affirms the contrary. But it is obvious one cannot continue to think of each mind as resembling white paper or a *tabula rasa* without by and by finding oneself influenced more by the suggestion therein contained of their similarity than of their dissimilarity. It is absurd to compare individual minds to pieces of white paper unless they possess a striking likeness. And undoubtedly the reason Locke attributed so much importance to education was because he was not impressed, as we are, by the great diversity in natural talent, industry and temperament which marks children. He confidently maintains that "nine Parts of ten [*i.e.*, nine men out of ten] are what they are, good or evil, useful or not, by their Education"[1]. "We are born with faculties and powers," he says elsewhere, "capable almost of any thing"[2]. He specially discusses one important respect in which he believes all men stand on an equality,—their faculty of reason seldom or never deceives them when they trust to it[3]. Quick points out[4] that in Locke's correspondence with Molyneux, he even goes so far as to maintain that intelligent honest men cannot possibly differ (see L. to W. M., 26. xii. 1692 and 3. ix. 1694, *Works*, IX. 300, 342). It is interesting, too, to note Locke's view on an allied matter. "Men," he thinks, "have been much the same for natural endowments in all times"[5].

---

[1] *Essay*, I. 2. 22, F. 1. 87.

It is therefore not surprising that the French philosophers took his figure of the blank sheet more literally than he intended, and denied the existence of any innate capacity or disposition[6]: accepted literally and uncritically, his doctrine of development suggests that the differences between men are purely the result of accident, or at least of the fortuitous circumstances of each man's experience. It is clear that our philosopher did not mean to go so far as this.

B.

Having considered Locke's views on the General Influence of certain specific studies, as well as some other points necessary to a proper realisation of his attitude towards the various aspects of the problem now described as Formal Training, we must next try to ascertain whether it is fair to say that he was one of those who place the emphasis in Education, not on the subject and activity learned or practised, but on the process of learning or practising. But before doing so, it will be well to discuss briefly Monroe's statement, to the effect that "the one fundamental thing that makes Locke a representative of the disciplinary education throughout is his idea of the human mind as a mere blank to begin with that has its virtues and powers worked into it from the outside through its formation of habits"[1]. We have had reason in the preceding section to modify the opinion that Locke looked on the human mind at birth as a blank, but even were this a literally correct expression of his views, it is not the fundamental fact that must be taken into account in any consideration of his attitude towards the disciplinary effects of education. In fact, we have seen that he sets up a vigorous opposition to such an opinion as that the memory at any rate can be moulded and trained for general effectiveness by learning Latin or any such subject. If he is to be classed as a disciplinarian at all, it must be because he believes the mind can, in the learning of one subject, be given a marked capacity and a strong bent for dealing with dissimilar subjects. The belief that the mind is at first a blank, and that it only subsequently gets its ideas, can conceivably be held along with the view that each idea must be got separately, and must be separately compared and connected with others, but that beyond this they do not affect each other. The disciplinary conception of education does not owe its peculiar characteristics to any view of the origin of ideas, but rather to a particular view of their mental interaction and reaction.

---

[1] *Thoughts*, § 1, p. 1. [2] *Conduct*, § IV, p. 13. [3] *Op. cit.* § III, p. 7.

[4] *Educational Reformers* (1898), p. 221. [5] *Conduct*, § XXIV. p. 52.

[6] *A Short Hist. of Education* (1919), J. W. Adamson, p. 204.

Another point it will be well to emphasise is that Locke disagrees with many 'disciplinary' educationists in not advocating the same general, formal course of instruction for all children, regardless of their future occupations and callings or of the length of time they are likely to remain in school and college. This is made clear in the "Epistle Dedicatory" found in the *Thoughts*, in § 217 of the same work, and elsewhere. Furthermore, a consideration of Locke's various statements also shows that he is fully aware that, before a person can reason accurately on any matter, he must be in possession of the relevant data, and our author knows that a correct decision sometimes requires the aid of facts and experiences drawn from several sources. Again, he is fully cognisant of the need for accustoming the mind to the methods adopted in different fields. He therefore recommends each person who has time and opportunity to "exercise the freedom of his reason and understanding in a [wide] latitude"[2]. He discusses the question in full in § XIX of the *Conduct*, where he places special emphasis on the gain the understanding receives from exercising it in various ways of inquiring and reasoning, and the danger of 'stiffness' from which it is thereby saved[3].

---

[1] *Text-Book in the Hist. of Education*, P. Monroe (1906), p. 513.

[2] *Conduct*, § III, p. 12.

[3] *Op. cit.* § XIX, pp. 43, 44.

Although he had not failed to make the observation referred to at the end of Position 5, that some educated people sometimes deal with new and difficult problems in a far more effective way than an untrained person does, yet he had no theory of mental 'dispositions' or 'traces' or of the subconsciousness to justify him in supposing that forgotten knowledge continued to affect the mind's operations. He speaks of "ideas lodged in the memory, and upon occasion revived by the mind"[1], of ideas "laid up in store"[2], etc. He had no conception however corresponding to our notion of the "intelligence of the subconscious." The nearest approach he makes to such an idea, so far as I know, is in the *Essay*, II. 10. 10, where he says birds "have perception, and retain ideas in their memories, and *use them for patterns*"[3] (my italics).

He could therefore only suppose that although some items of knowledge had dropped into oblivion, the mind had, in the process of acquiring them, got stronger. There is of course no reason why Locke or any one else should not speak of the mind gaining strength and suppleness, so long as it is realised that such an increase means, for one thing, that many ideas having been worked over and over in full consciousness, and having dropped into the subconscious, continue to function there, and for another thing, it means that the mind has gained some ideas which it can, when it likes, call up and manipulate. Not having an insight into the first matter, he could not be expected to understand the second, inasmuch as they depend so intimately the one on the other, or rather since they are only aspects of one phenomenon. I repeat therefore that the only way in which he clearly perceived the mind to benefit, after it had forgotten the experiences it had once worked through, was by postulating an increased strength won in working through them. Of these two factors by which the mind is affected in its attempt at solving a particular problem, or meeting a situation, namely, that of past and forgotten experience on the one hand (taking the form, in his theory, of increased mental power), and that of the ideas it consciously uses on the other, he attached far greater importance to the former, although I have shown that at times he considered the latter absolutely necessary. We can therefore understand why he makes the recommendation I have previously referred to. Men, he says, "should be made to look into all sorts of knowledge, and exercise their understandings in [a] wide

---

[1] *Essay*, II. 10. 7, F. I. 198.

[2] *Op. cit.* II. 10. 8, F. I. 199. Also II. 10. 2, F. I. 193–4.

[3] A Professor of Philosophy who was good enough to read this article in MS. for me, asks whether Locke does not mean that this takes place when the ideas are revived.

variety and stock of knowledge. But I do not propose it as a va
and stock of knowledge, but a variety and freedom of thinking, a
increase of the powers and activity of the mind, not as an enlarge
of its possessions"[1]. We must therefore note that just as we think
our legs, by the training got in walking to a place of business
preparing for a football match, gain strength and flexibility for wal
and running, whatever the place or time in which we wish to em
them, so the mind, in Locke's opinion, by the work it does in lea
Latin or French or any other subject, although it afterwards forge
about them, increases its capacity for dealing with any and e
question it has to face. This is his theory: it is a theory of Fo
Training. But it must be equally clearly recognised that it is in contr
tion with his recommendations on educational practice. He now
recommends the study of any subject (except one perhaps) mere
an exercise for the mind: if an exception must be made, it is mathem
and even in that case, he does not forget to refer to its high pra
value, and although he claims for it a special worth as a means of
cising the mind, yet his advocacy of a privileged place for it is as
based on the consideration that it is a type to which arguments on
matters should be assimilated as on its value as a gymnastic.

Instruction is however, in Locke's opinion, only a small pa
Education. "Learning," he says, "must be had, but in the second P
as subservient only to greater Qualities"[2]. Indeed Learning is a
help to Virtue and Wisdom in well-disposed minds[3]. What then
Tutor's main business? Locke's famous answer is the following:
great Work of a Governor, is to fashion the Carriage and form the M
to settle in his Pupil good Habits and the Principles of Virtue
Wisdom; to give him by little and little a View of Mankind, and
him into a Love and Imitation of what is excellent and praisewo
and, in the Prosecution of it, to give him Vigour, Activity and Indu
The Studies[4] which he sets him upon, are but as it were the Exer
of his Faculties, and Employment of his Time, to keep him
Sauntering and Idleness, to teach him Application, and accustom
to take Pains, and to give him some little Taste of what his own Ind
must perfect"[5]. Here again a Formal Type of Moral Education s
to be recommended. The tutor will, it is claimed, by instilling int

---

[1] *Conduct*, § XIX, pp. 44, 45. [2] *Thoughts*, § 147, p. 129. [3] *Ibid.*

[4] It will be remembered that he nevertheless proceeds later on in the *Thoughts* t
with much feeling and thoroughness with the subjects his pupil should, and should not

[5] *Op. cit.* § 94, p. 75.

pupil a love of what is excellent, and securing from him an imitation of it, give him Vigour and Industry; which qualities the philosopher imagines will exhibit themselves with considerable, if not with equal, force in all departments of life. But when we turn to the methods he puts forward for fashioning the pupil's 'carriage' and settling in him good habits, for forming the mind and planting in him the principles of Virtue and Wisdom, we do not find any one special method selected for doing this work. Instead of that, we find that every act and motion that go to form the 'carriage' must be practised until each of them falls naturally, "as skilful Musicians' Fingers do, into harmonious Order without Care and without Thought"[1]. Locke's treatment of the methods to be adopted in forming Habits makes it clear that he would agree that, just as the musician has to practise each finger separately and in conjunc tion with the others, in order to secure command over a particular instrument, so each act and movement included in a habit will require individual and collective exercise. He has no specific for the formation of the 'Carriage' such as Sloyd has, by some, been supposed to be for the performance of manual acts. Furthermore, he warns us that "from repeated Cautions and Rules, never so often inculcated, you are not to expect any thing either in this, or any other Case, further than Practice has establish'd them into Habits"[2]. Each habit must by example and use be woven into the very Principles of the Child's Nature[3]. "What you think necessary for them to do, settle in them by an indispensable Practice, as often as the Occasion returns"[4].

Also in his discussion of the ways of forming the 'carriage,' he does not forget to deal with the inculcation of right principles and notions. He does not despise the aid of Precepts: he definitely says that in aiming at Virtue, Tutors should read Lectures on it and talk about it[5]. In Morality, Prudence and Breeding, 'Cases' should be put to the pupil and his Judgment on them asked[6]. He should also exercise his Reason in discussing fit Instances with his Tutor[7]. It is thus clear that Locke rightly looks upon Habit, not as a specific response to a specific stimulus, but as a more or less similar response to a more or less similar situation. At first, each response and situation dealt with will be as simple and free from complexity as possible, and must be repeated till the necessary firm connections have been set up between them. Many allied and slightly different responses and situations are next treated, so that at

---

[1] *Op. cit.* § 93, p. 70.
[2] *Op. cit.* § 10, p. 7.
[3] *Op. cit.* § 42, p. 28.
[4] *Op. cit.* § 66, p. 39.
[5] *Op. cit.* § 70, p. 50.
[6] *Op. cit.* § 98, p. 80
[7] *Op. cit.* § 98, p. 81.

9K296

last what Locke would call a Habit, for example, a Habit of Industry, is established. That he fully realises the complexity of the task involved in cultivating such a Habit may be seen from his detailed discussion and analysis of Courage (*Thoughts*, § 115) or Industry (see under 'Sauntering,' § 123). He points out that in attempting, for instance, to develop Courage, Natural Temper plays a considerable part: few men attain to true Fortitude, and so we cannot expect it from children, "but yet something may be done"[1]. I repeat that he has no kind of study or exercise to recommend as a panacea for all youthful infirmities, or a specific for the cultivation of every virtue.

The word 'discipline' is continually in our author's mouth: pupils must be hardened in mind and body; they must learn to deny themselves; their minds must be rendered obedient to discipline and pliant to reason[2]. It is therefore justifiable to say that his conception of education is in one sense a disciplinary one, but in another important respect it differs *toto caelo* from the usual interpretation of what constitutes 'formal discipline' or 'mental training' or 'mental discipline,' in that he attempts to modify action and form character, not by one or two special studies and exercises, but by the unceasing repetition of hundreds of individual acts, close imitations of selected patterns of many movements and conduct, and friendly discussions of 'cases' and rules and principles, so as finally to develop a strong tendency to perform actions on the general lines of those previously recommended, and to instil a love for virtue and an appreciation of the difference between right and wrong. He undoubtedly expects more in the way of the development of Industry, Courage, etc., from education than most educators do to-day: we are far more conscious than he was of the subtle and powerful directions in which heredity defies the teacher to alter, much less to eradicate, certain tastes, passions and impulses to action, implanted in our pupils even before they are born.

I therefore conclude that it is a serious misrepresentation of Locke's views to say that he places more emphasis on the process of learning than on the subject learned. It is however true to say that he did not give a prominent place to Knowledge as a factor in *Moral* Education. He thinks that Learning must be had, but he only gives it a second Place: it is, he says, a great help to Virtue and Wisdom, but he had no conception, such as Herbart for instance had, of the way in which a study of Literature or History or Art may be made to colour the mind and change its attitude towards life. It is only in one place that Locke seems to acknowledge the great influence of ideas, namely in the opening words of the *Conduct*. "The last resort a man has recourse to," he says, "in the conduct of himself is his understanding; for though we distinguish the faculties of the mind, and give the supreme command to the will as to an agent, yet the truth is, the man which is the agent determines himself to this or that voluntary action upon some precedent knowledge, or appearance of knowledge, in the understanding....Temples have their sacred images, and we see what influence they have always had over a great part of mankind. But in truth the ideas and images in men's minds are the invisible powers that constantly govern them, and to these they all universally pay a ready submission." He failed however to see that it is one of the teacher's main and not subservient functions to select the ideas that are to be presented to the pupil, to help him in understanding, systematising and applying them, and in the process to evoke such pleasure and interest as to secure for them then and afterwards a free and open road to action. But to point out this defect in his theory is not to charge him with a failure to realise that certain subjects must in the first place be studied for their intrinsic value.

---

[1] *Op. cit.* § 115, p. 95.

[2] *Op. cit.* § 34, p. 21.

CONCLUSIONS

1. Locke never considered or advanced any explicit Theory of Formal Training.

2. His rejection of the Faculty Psychology is complete. (It has not been thought necessary to discuss this point above.)

3. In his psychology, he gives no consistent answer to the question whether the mind can transform and transmute the ideas it receives. In the main, he proceeds on the view that all the mind can do is to compare, contrast, and enlarge its experiences. His treatment of universals is fatally defective.

4. In respect to one mental process, namely, the Memory, he explicitly says that it cannot be improved, save in so far as Method and Order are introduced into the arrangement of the material to be memorised:

5. Most of his teaching points to the conclusion that Attention also cannot be strengthened, but he does not say so definitely. Nevertheless, he speaks of cultivating a Habit of Attention which is presumably to operate in a general way.

6. He clearly recognises that the influence of one experience or class of experiences is not confined to those of the same class, but tends to affect others. He is deeply concerned with the injurious consequences

of a general character induced by long application to only one subject or art.

7. He is anxious that care should be taken, in educating a pupil, to give him, while he is young, a taste for several subjects and an insight into the methods employed in them, and in this way to open his mind so as to prepare him to apply himself to one or more of them later on, if that should be found necessary.

8. In the selection of these subjects, he gives the preference to those that will be of the greatest use to the learner in after life. He is guided by utilitarian considerations, and not by those of formal discipline. The pupil's after-school occupation must be kept in mind.

9. He does not think difficulty adds to the value of a study. Indeed, he believes the teacher must take pains to find out the methods that will make the process of learning pleasurable and interesting, and give the most rapid mastery over the subject studied.

10. Strangely enough however, when he stands back from considering at close range the way in which the individual subjects should be taught and the reasons for including them in the curriculum, and takes a broad view of the Aim of Education as a whole, he concludes that every subject learnt is, as it were, but a medium for exercising the pupil's mind and employing his time. On the other hand, he realises that if a person is to form correct judgments on the many situations he will be confronted with, he must study many subjects: nevertheless, Locke does not propose this in order to add to the variety and extent of the individual's knowledge, but as a means of increasing the power and activity of his mind. It will be thus seen that Locke's views on the relations that exist between effective thinking and wide mental possessions are vague and unsatisfactory.

11. He saw that there were great initial differences between men's temperaments, but he did not realise equally clearly and steadily that there are serious congenital inequalities in their intellects too. In his great polemic against the dogma of innate ideas and principles, he found it necessary to emphasise the fact continually that we are not born in possession of even one complete idea, and that, in this respect, a mind may be compared to a sheet of white paper, on which not a single word has been written. To compare minds however to sheets of white paper almost inevitably leads to thinking of them as practically identical with one another. He seems also to have proceeded on the assumption that the average mind possesses a good measure of ability. These various considerations led him to assign more influence to Education than it can properly lay claim to.

12. In the recommendations he makes on the Physical Training of the child, he wishes that the little one should be accustomed to bear hardship and pain. Likewise, in the advice he gives on Moral Training, he recommends that the child should learn to deny himself all that he particularly desires, and cultivate habits of restraint, courage, vigour, industry, etc. The aim Locke sets before parent and educator is therefore of a general character. The training given in moral education must be a discipline which will function not only in situations of an already experienced type, but in others differing somewhat from them.

But Locke knows of no specific for producing this result. Action after action, example upon example, 'case' by 'case,' must be practised, imitated, and discussed before even one habit becomes firmly rooted. Nor can success be at all times assured: the original nature may be weak and shifting, or the situation to be faced baffling and complex, and consequently failure or despair may ensue.

In no part of the advice he gives on Moral Training, does he include the necessity for creating strong intellectual, aesthetic or practical interests.

13. Locke lived in an age which did not concern itself with training experts. Many sciences had not then reached a stage of complexity in any way approximating that to which they have now attained. It was at that time easy to hold the view that after a pupil had studied several subjects under the guidance of a tutor for several years, he could without much difficulty proceed to master their more advanced portions by himself.

14. Locke, like Bacon, Descartes and Spinoza, concerned himself closely with the search for some principle or principles by which it could be decided whether a body of knowledge was valid or not. Such principles he thought he had found in the Mathematical Sciences. When he came to write the *Conduct*, he recommended all who had leisure to master these, because the 'habit' thus got of examining and arranging premisses, and drawing conclusions from them, would transfer itself to the treatment of arguments dealing with other topics, especially since, in his opinion, every argument, taken singly, can be arranged in a mathematical way. No consideration of this kind is however put forward in the *Thoughts* as a reason for learning Arithmetic and Geometry: they must be studied for the sake of their high practical value. But it is very probable that Locke did not believe the general results referred to in the *Conduct* could be obtained from a study of mathematics pursued only for a few years.

(*Manuscript received* 1 *November* 1921.)

RECENT CONTRIBUTIONS TO THE THEORY OF 'TWO FACTORS[1].'

BY C. SPEARMAN.

§ 1. It is a great pleasure to express my welcome of the ingenious contributions to this theory that have recently emanated in large number and diverse places from Prof. G. H. Thomson, written as he impressively remarks "with all the emphasis of which (his) pen is capable," and culminating in two chapters of a very important book of which he is part author[2].

As regards the grave opposition of all this work to some attempted researches of my own, I have considered that the wisest use to be made of it, and at the same time the best compliment I could pay to it, would be to take it as a stimulus to further researches on each chief dubious point. And these are actually in progress. Still, in order not even temporarily to seem heedless of such formidable criticism, some few remarks may be adventured at once.

The main argument of Thomson, if I understand him aright, is that the correlation between the arrays in the table of the inter-correlations of a set of mental tests ought not to be attributed (as done by me) to any 'general factor.' For such a correlation, he says, should be expected to arise in any case by mere chance.

But here, as upon a previous occasion[3], I must observe that his dart is feathered from my own plumage; such a chance correlation was long ago announced by myself[4]. Moreover, it seems to me that, while so bent, he ought at least to have plucked a few neighbouring feathers as well. My paper not only mentioned the said correlation, but even suggested its amount. And the latter is so low as to render his whole objection nugatory.

---

[1] This little paper was shown to Prof. Nunn, who kindly made such useful and elucidating comments, that the liberty has been taken in two or three passages of replacing the original words by his.

[2] *The Essential of Mental Measurement*, 1921. In these two chapters, his colleague, Dr W. Brown, does not appear to have taken part

[3] *This Journal*, VIII, 1916, p. 282.

[4] *Psycholog. Review*, XXI, 1914, p. 109.

§ 2. In return for his shaft, I should like to let fly one myself, this time feathered from his own tail. His second main point, as I interpret it, rather disconcertingly shifts from *any* correlation between the arrays to the extremely high correlations of this kind that have been called 'hierarchies.' Even these high correlations, he convincingly demonstrates, can be obtained by means of his ingenious procedure with dice, without, as he plausibly contends, introducing anything in the least resembling a general factor. He concludes that the mere fact of a good hierarchy occurring cannot possibly anywhere constitute a proof that a general factor exists.

There is, however, one case in which he himself most deliberately and positively concedes that a hierarchy *would* prove the existence of a general factor, namely, the case in which the hierarchy is perfect. An entire paragraph in a sort of formal profession of faith (intended to set forth how far he agrees with Maxwell Garnett) runs as follows:

"If the hierarchy is perfect there is one and only one general factor g and no group factors, but there may be any number of specific factors in addition[1]."

Now, I have shown that by using Thomson's own formula, which does indeed *seemingly* involve no general factor, it is still possible to produce a quite perfect hierarchy[2]. Consequently, according to his own faith, his dice hierarchies do, after all, introduce a general factor, albeit surreptitiously. His work is really a new corroboration of the thesis, that good hierarchies indicate a general factor. The theory which he went out to curse, he ends—unawares—by blessing.

§ 3. It may be of interest to explain how this trick is managed of introducing a general factor whilst seemingly not doing so.

Suppose any number of test results (or other variables) to be each composed of, and straight forwardly expressed in, one constituent common to all (the 'general factor') and one or more specific to itself. We thus get

$$\begin{aligned} a &= g \times w_a + s_a, \\ b &= g \times w_b + s_b, \\ c &= g \times w_c + s_c, \\ &\ldots\ldots\ldots\ldots\ldots\ldots \end{aligned}$$

where g is the constituent common to all the tests and varying only with the individuals; the w's are the weights with which it enters into the different tests respectively; whilst the s's are the specific constituents varying in all tests independently both of one another and of g.

---

[1] *This Journal*, IX, 1919, p. 368.

[2] *Psycholog. Review*, XXVII, 1920, p. 164.

We can immediately hide the common constituent out of sight simply by splitting $g \times w$ into any two parts, which we will write as g' and g''. In this way

$$\begin{aligned} a &= (g'_a + s'_a) + (g''_a + s''_a), \\ b &= (g'_b + s'_b) + (g''_b + s''_b), \\ c &= (g'_c + s'_c) + (g''_c + s''_c), \\ &\cdots\cdots\cdots\cdots\cdots\cdots \end{aligned}$$

Nothing prevents us from continuing in a still more artful manner and so arranging our division that

$$\begin{array}{llllll} (g''_a\, s''_a) & \text{is equal to} & (g'_b\, s'_b) & \text{and is called} & x, \\ (g''_b\, s''_b) & \quad ,, & (g'_c\, s'_c) & \quad ,, & y, \\ (g''_c\, s''_c) & \quad ,, & (g'_a\, s'_a) & \quad ,, & z. \\ \cdots\cdots & \cdots\cdots & \cdots\cdots & \cdots\cdots & \cdots \end{array}$$

To achieve the finishing touch, the division must have been effected in such a peculiar manner, that our eventual x, y, z ... are mutually uncorrelated[1]. At this final point, we have reached

$$\begin{aligned} a &= x \phantom{{}+y} + z, \\ b &= x + y \phantom{{}+z}\ , \\ c &= \phantom{x+{}} y + z, \\ &\cdots\cdots\cdots \end{aligned}$$

where x, y and z are uncorrelated.

And this is precisely the form of Thomson's hierarchies obtained by dice. Nothing whatever remains visible of the common constituent; it can at this stage only be inferred from the fact of a, b and c still retaining all the remarkable mathematical relations to one another which they had originally in consequence of the single common constituent. Not less remarkable relations (such as their number, inter-arrangement, standard deviations, etc.), inevitably characterise also x, y and z, in spite of their being in a certain sense mutually independent.

In what manner, now, does all this shuffling genuinely affect the original single common constituent g? Of a certainty, the latter has never been eliminated. It has only been split into parts, manifesting itself in one variable as g'_a and g''_a, in another as g'_b and g''_b, ... Suppose any pack of cards to be on different occasions dealt out differently along with some further cards. Nobody would deny that it still remains all the time the very same pack.

---

[1] The possibility of doing so in general can be at once proved by comparing the number of conditions to be satisfied with the number of arbitrary variables.

§ 4. A most important point about this whole shuffling operation consists in its being *reversible*. Just as the general factor can be buried away till no vestige of it remains visible except certain peculiar mathematical relations, so too, whenever such relations hold good, there the general factor can actually be unearthed again. Garnett has demonstrated this reversibility in the special case of 'normal' distribution of frequencies and an infinite number of variables[1]; and I have recently shown that it is valid for any distribution and any number of variables whatever[2].

Let us pick out any instance from that hierarchy of Thomson himself, in respect of which he has published his method of procedure in detail[3]. Thus, one of his variables, called by him k was the sum of the throws of the 9th, 14th, 16th, 20th, 21st and 30th dice. This fact may be written as follows

$$k = d_9 + d_{14} + d_{16} + d_{20} + d_{21} + d_{30}.$$

But as proved by me, these equations admit of being re-written with approximation (*not* with exactness, since the proportionality between the arrays was imperfect), as

$$k = (g_9 + s_9) + (g_{14} + s_{14}) + (g_{16} + s_{16}) + (g_{20} + s_{20}) \\ + (g_{21} + s_{21}) + (g_{30} + s_{30}).$$

Here $$g_9 + g_{14} + g_{16} + g_{20} + g_{21} + g_{30} = g \cdot w_k.$$

and $$s_9 + s_{14} + s_{16} + s_{20} + s_{21} + s_{30} = s_k,$$

g being common to all the variables alike, w_k being a constant for the variable k, and s_k being independent both of g and of all the other analogous s's. The w's can at once be actually calculated; and so could the g and the s's if the dice-throws were given. It may be added that even the liability of the general factor to be itself thus split up into a more or less elaborate group of components is not new; it was already stated in the very first publication on the whole topic[4]. Even this, then, I must reclaim as one of my own feathers.

§ 5. To sum up, Thomson's language seems to suggest that the terms of his hierarchies are produced entirely by chance. This is not the case. It is true that he obtains by the throwing of dice (*i.e.* by chance) the components from which his terms are obtained by summing. But he himself determines the number of these terms, their position in the tabular arrangement, etc.; and he determines these things deliberately with a view to obtaining a hierarchy. If he did not manipulate his chance data with this intention, but instead allowed chance to determine also the number, positions, etc. of the data, then there would only ensue the low correlation between the arrays already long ago predicted by myself. And his very manipulation of the data amounts, in the way shown above, to the introduction of a general factor.

---

[1] *Proc. Royal Soc.* 1919.

[2] To appear in the next *Proc. Roy. Soc.*

[3] *This Journal*, VIII, 1916, p. 277.

[4] *Amer. Journ. Psych.* xv, 1904, p. 284.

It is gratifying to be able to close these few remarks by stressing one fundamental fact upon which I like to think that Thomson and myself are in complete accord. If here the mathematical conversions have had the power of disguising this common constituent of a, b, c, ... in terms of x, y, z, ..., the same power is also possessed by nature. In the real world, nothing prevents the common constituent from having been cut up and portioned out into just these most peculiarly inter-related conglomerates x, y, z, ... But for my part, I do plead that before believing in such a mysterious disintegration and re-integration, we may fairly demand some sort or other of evidence for them[1].

---

[1] *How* Thomson arrives at all his misapprehensions, namely by never defining the nature of the "single general factor" which he tries to disprove, has recently been set forth by that fine mathematician Maxwell Garnett (*Brain*, 1921, XLIV, pp. 332 ff.).

(*Manuscript received* 17 *March* 1922.)

SOME PROBLEMS OF ADOLESCENCE[1].

BY ERNEST JONES.

1. *Introductory* (pp. 31–33).
2. *In what does 'growing up' consist? Five features (a–e) of difference between children and adults* (pp. 33, 34).
3. (*a*) *Intellectual development* (pp. 34, 35).
4. (*b*) *Integration* (pp. 35, 36).
5. (*c*) *Emotion and imagination* (pp. 36, 37).
6. (*d*) *Dependence* (pp. 37, 38).
7. (*e*) *Sexual maturity* (pp. 38, 39).
8. *Adolescent recapitulation of the phases of infantile sexual history* (pp. 39–41).
9. *The above-named features (a–e) re-considered from the standpoint of recapitulation* (pp. 42–46).
10. *The biological significance of puberty from this standpoint* (p. 47).

1.

BEING deeply sensible of the responsibility and honour bestowed on me by being asked to address this joint meeting, I have reflected that the subject of my remarks should be one worthy of the importance of the occasion, and also one of sufficiently wide bearing to insure the interest of workers in different fields. One possible source of misapprehension in my title should be cleared away to begin with. It concerns the use of the word 'problem' in this connexion. Many of you, particularly perhaps among the members of the Educational Section, may expect that with this word I have in mind something of an executive order, and that I intend to convey my opinions on how best to deal with various 'problems' facing the teacher, for instance, or with the guiding of adolescents through various 'problems' or with the difficulties with which he is likely to be faced. Such a paper would presumably be called a 'helpful' one, but I am afraid I shall disappoint those of you who look for one of this kind. The sense in which I here intend to use the word 'problem' is that of a nodal point of ignorance, to which an 'answer' consists of a correlation, one which for popular purposes is conveniently expressed in terms of what is called causation.

---

[1] Delivered before a Joint Meeting of the General, Medical and Education Sections of the British Psychological Society, March 14th, 1922.

It is worth dwelling a little on the difference between the two points of view just indicated, for it has a definite bearing on most of the questions of interest in connexion with adolescence. As is well known, the special danger besetting all forms of applied science is that too much emphasis is apt to be laid on the 'applied' and too little on the 'science.' With all the affairs of life that deeply engage our interest—for instance, those of health, of education, or of politics—the impulse to do makes us impatient of the desire to know. We usually prefer to act without knowledge rather than to seek knowledge first and act after; we may at times be willing to make use of knowledge if it can be acquired without too much effort, but only too often the insistent necessity of acting somehow at all costs is paramount over the knowing precisely what the results of the action may be.

In contrast with this instinctive tendency the object of science is knowledge and nothing else whatever. Once knowledge has been attained it can of course be subsequently taken into account when deciding on this or that plan of action, but this involves a quite different category of mental activity which it is expedient to keep distinct from the former. To come nearer to our subject of to-night: it occurs to me that there must exist already a number of societies, congresses, clubs and magazines in sufficient profusion to afford abounding opportunity for the discussion of the art of dealing with adolescence, educationally or otherwise, and that there is ample room for one society, such as ours, whose sole aim should be the pursuit of knowledge for its own sake, where the desire to know should be allowed full sway with the temporary abrogation of all other impulses. The bearing of these considerations will, I trust, become evident in the remarks to follow.

A second expectation more general among my audience I do not propose to disappoint. It will be anticipated that no psycho-analyst is likely to treat of adolescence without mentioning the word 'sex,' and to do so would, in my opinion, be like playing *Hamlet* without the Prince of Denmark. The educated world has agreed upon a conspiracy of silence about the sexual life of childhood, *i.e.* before puberty, and the subject is rarely referred to unless explicitly to deny its existence. Even the sexual life of adults is regarded as so unimportant as not to be worth discussing in most textbooks of psychology. Now if the sexual instinct ever plays a significant part in the life of man it surely is during the age of adolescence, for biologically the essential characteristic of this age is that it represents the achieving of sexual maturity. Few indeed are the problems relating to adolescence that can be solved without taking into account some sexual factor or other.

2.

There are innumerable detailed problems of adolescence to which time could profitably be devoted, but most of them are concerned with one or other aspect of the great developmental process that we call 'growing up.' It seems to me, therefore, most fitting that I should attempt here to consider the general nature of this cardinal process, for any effort to attain a better understanding of it must necessarily be of assistance in the study of the many other problems that stand to this one rather in the position of corollaries to a main proposition.

I wish, therefore, to ask in what precisely does this 'growing up' consist, what are the forces bringing it about or impeding it, and to what extent, and with what results, it is usually achieved. What is actually effected during this transitional stage of adolescence, when the child becomes the man? Perhaps we can best approach this question by inquiring into the chief differences between the beginning and the end of the process, between the child and the man. There are, of course, many such. Some of them are less valuable for our purpose, for they are only very gradual in kind and are largely matters of increasing age and experience. Others are more characteristic and justify us in determining the extent to which a given person has grown up, the extent to which he has cast off or retained childish traits, and it is to these differences that we have to pay special attention. Not that they also are not gradual in kind, for Nature makes no leaps here any more than elsewhere, but the change is sufficiently striking to enable us for practical purposes to distinguish them in another class from the former ones.

One might think that it would be no very formidable task to establish the most notable differences between a child and an adult, but I can only say that on attempting it I found it extremely difficult. Many of the points that at first sight seemed secure enough turned out on closer inspection to be more than uncertain. Often one knows perfectly well whether a given reaction or capacity is more typically childish or grown up, but to define the essence of it is quite another matter. What I have to put forward for your consideration here, therefore, is especially tentative, and I will ask you to receive it as such.

Closer examination of the difficulty of this comparison shows that, as is often so, the matter has not been justly stated. We are not comparing sufficiently pure elements. The whole of life up to the time of puberty is not a single element, nor is the whole of it after this date. Both periods should be further divided into two, the former into infancy and late childhood, the latter into adolescence and fully adult life[1]. If we contrast late childhood with adolescence we do not come to at all the same conclusions as we do if we contrast the other, earlier period of childhood with the other, later period of adult life. Reflexion along these lines will, I maintain, show that it is, on the other hand, infancy and adolescence that have to be contrasted with late childhood and adult life. In order to secure the most pronounced contrasts for our present purpose, therefore, I will mainly consider those between the two extremes, infancy and fully adult life.

3.

Let us begin with one of the more obvious differences, namely the greater development of intellectual powers in the adult. This is the aspect on which I feel personally to be least qualified to speak, and I have a becoming respect for the enormous amount of experimental research that has been carried out on this subject. Scales have been established by means of which the progress of intelligence from year to year can be estimated and the intellectual age of various individuals compared with their actual age. Efforts have also been made by Burt, Yule and others, to devise methods for isolating the common factors in the different forms of intelligence testing and for estimating the correlation between these and other mental traits. But with all deference to this work, I wish to submit two remarks of a sceptical nature in regard to some of it. Rightly or wrongly, I feel convinced that there yet remains much to be done in the way of critically examining the nature of many of these tests from the purely psychological side. They are, I think, too often regarded from an external and matter-of-fact point of view, often from an exceedingly adult point of view, without due consideration being paid to the very subtle ways in which the reaction to the tests may be influenced by delicate affective, and commonly quite unconscious, factors. Speaking more generally still, I would say that my psycho-analytic work has given me a very high opinion of the intelligence of children and has taught me that there are some important respects in which it commonly becomes positively blunted by increased age, or rather by that 'education' which is an unescapable accompaniment of this increase. I have been impressed over and over again by the bold initiative of thought, by the sceptical and searching spirit of inquiry that can be displayed by a young child, of which the same person never shows a trace throughout all his later life. It is true that one has to set beside this the child's suggestibility and credulity—though the latter is often more apparent than real—and the effects of these soon put an end to his earlier freedom of thought. Nevertheless, this sort of experience does suggest to me that much of the experimental work on intelligence needs a further analysis of the nature of the elements that are being measured and of the possibly different processes that may be included in the category of intelligence. It is remarkable how little we really understand of the ideational aspects of the mind when one reflects how exclusively the attention of psychologists has for centuries been concentrated on these aspects: possibly, indeed, it is just because of this exclusive concentration that we know so little.

---

[1] I use the word 'infancy' to cover the years up to five, 'childhood' those up to twelve, and 'adolescence' from twelve to eighteen. Twelve is chosen rather than fourteen on the ground that the changes incidental on puberty show themselves earlier in the mental than in the physical sphere.

4.

The *second* feature that occurs to me as distinguishing the adult from the child is the higher degree of what may be called integration in the former. By this I mean a greater coordination among the various component parts of the mind. With adults the mind seems to work more as a whole than with children, so that a given action is more likely to be related to the rest of the personality than it is with children, where it may be mainly the expression of a single more or less disconnected impulse. Something can be said about the nature of this integration. It is essentially dependent on a heightened capacity for inhibition, as may be seen if we contrast the extreme cases of a deliberate action in a well-balanced adult and the impulsive conduct of the infant. (In later childhood there is often a very considerable degree of integration, often much more than in the immediately succeeding years of adolescence; this correlation between infancy and adolescence on the one hand, and between later childhood and adult life on the other, is one to which we shall return later.) Now in the contrast we are depicting there are two prominent features. The impulsive action of the infant is often concerned only with the impulse itself, in it no account is taken of other tendencies or reflexions, and it approximates more or less closely to the dissociated, autonomous 'complex activity' so familiar in paraphrenia and other mental disorders. In the second place, it is obviously hard to control and clamours for *immediate* expression; the young child is notoriously extremely impatient of delay or postponement. I would venture to connect these two features and to suggest that the small extent to which the individual impulses are brought into relation with the rest of the mind in infancy is closely connected with the fact that the child has not yet learned to tolerate excitation without an immediate response, *i.e.* to be able to interpose a delay between the excitation and the motor or emotional response. What the precise connexion is between the increase of coordination and of inhibition is a matter calling for a more technical discussion than is possible here. I will only add that the greater power of inhibition, *i.e.* the capacity to endure a greater amount of psychical pain, is an essential part of the developmental process of subordinating the pleasure-pain principle to the reality principle.

The suggestion may be put forward at this point that a fuller understanding of this problem of integration will probably throw much light on the difference in intellectual power at different ages, for the higher reasoning powers, the capacity to think in general and abstract terms, seem to bear a close relation to the power of concentrating, a faculty in which coordination and inhibition play a very important part.

5.

The *third* distinguishing attribute is not such a compact one as the last. It has to do with certain features common to various emotional and imaginative traits in each class. If we compare the phantasy life in children and adults, especially young adults, it may at first sight appear to be a mental aspect equally developed in both, but there are certain characteristic differences in the two cases. The phantasies of childhood are much more obviously egocentric than those of later years. A young man or woman will often weave an elaborate fiction in which they do not recognisably appear, but with children the hero is hardly disguised if at all. Then, as a rule with children, we miss the note of yearning or aspiration, that reaching out to something beyond the individual self, an endeavour even to attain to the infinite, with which we are familiar particularly in late adolescence. It may assume manifold forms, religious, artistic, poetical, or purely social, but in all of them the characteristic mark is the feeling that the self is incomplete or even unsatisfactory and the intense desire to get into contact with something, an idea or a being, outside the self. With this goes usually a much greater development of altruism, and the essence of the whole group we are now considering may well be the development of a higher capacity for loving and the desire to love, as distinct from the older desire to be loved, which is such a prominent feature of childhood.

In this connexion we must also think of the relation of phantasy to reality at different ages. On the whole it would appear that the life of phantasy is gradually brought into closer relation with the facts of real existence at two different periods during the development of the individual, first, during the transition from infancy to later childhood, and then, more definitively, during the transition from adolescence to later adult life. This change also betokens a similar progression to the one we have just noted from preoccupation and satisfaction with the self to a greater interest and love for the outer world: in other words, an unfolding of the personality so that the flow of feeling outward away from the self is facilitated. Numerous differences of detail are of course to be correlated with this great evolution; I will only instance the greater selfishness and cruelty of children as compared with adults.

6.

A *fourth* group of differences comprises the more specific changes in the individual's relationship to his fellow beings. The most prominent feature here surely is the diminution in the degree of dependence shown. I purposely say diminution, for there are very few adults, if any, in which traces of the old childish feeling of dependence are not still to be found and quite pronounced degrees of it are common enough at all ages. The change represents one of the most important of all the differences between children and adults, and the extent to which it has been brought about is one of the criteria on which we most rely clinically in deciding how successfully a given person has performed the task of growing up. Relics of the older attitude are commonly seen, not merely in such gross manifestations as timidity or embarrassment in the presence of superiors, but in manifold forms of undue sensitiveness to public opinion, the opinion of the neighbours, or even of the servants. Or the person shows the need of outer support in everything he undertakes, he does not venture to rely on or even to use his own powers of initiative, he dares not act for himself, he dares not think alone, and in extreme neurotic cases he dares not even walk alone. The manifestations in the intellectual sphere alone are of infinite variety; all degrees of stupidity occur which are really expressions not of a defect, as they appear to be, but of inhibition. The number of people who can make a genuinely free use of their mental powers in thinking is extremely small.

Psycho-analysis of both the milder and the more strongly marked forms of dependence has done much to elucidate its nature. One conclusion stands out with especial clearness, that all forms of psychical dependence are fundamentally due to persistence of the infantile attachment to one or other parent, particularly to the fear of being disapproved of by that parent and thereby losing his or her love. The change from dependence to independence, which is such a cardinal constituent of the process of growing up, is thus seen to be closely related to the question of attachment or detachment in reference to the parents.

The character traits of enterprise, responsibility, initiative, and self-reliance are as a rule distinctly more pronounced in the male sex. Furthermore, of the external influences that can affect their development, the most striking are experiences in the sexual sphere, notably the acceptance of masturbation and the abandoning of the early sense of guilt in connexion with this, the first act of coitus, marriage, and the birth of the first child; the death of the significant parent is also to be noted as often playing an important part here. These considerations alone suggest that possibly the phenomena in question are connected with the psycho-sexual life, and we shall presently see that there are other grounds for coming to this conclusion.

7.

We thus come to the last, but by no means the least, of the features we are enumerating, namely the fact of sexual maturity. This is the most outstanding and undeniable difference between children and adults, and we have therefore to define it more closely. Not long ago this would have seemed a very simple task. It would have been said that the difference consisted in the absence of sexuality in the child and its presence in the adult. Since the researches of Freud and his school, however, this position is no longer tenable. We have now to recognise that there exist in all societies strong motive forces impelling adults on the one hand to restrict in every way possible the manifestations of sexuality in childhood and on the other to blind themselves to the sexual nature of these manifestations; between these two methods the recognition of infantile sexuality has been reduced practically to a vanishing point. How far this attitude on the part of adults represents a useful biological function or is merely a sociological phenomenon, originating in jealousy and perpetuated by repression, is an important question which has been interestingly considered by Mr Flügel in his recent book *The Psycho-Analytic Study of the Family*. What we have here to consider is the nature of the transition in the sexual life that occurs during the development from child to man.

As is well known, the conclusions arrived at by psycho-analysts on the subject of sexuality in childhood are as follows. During the first four or five years of life the sexual life of the child, both physically and mentally, is an exceedingly rich one and passes through a series of characteristic stages; the nature of these stages will be presently alluded to. Its manifestations differ very considerably from those familiar in adult life and resemble rather those of primitive man or of our pre-human ancestors; they are thus decidedly atavistic in type, an additional reason why their sexual nature has been so extensively overlooked. When the second half of childhood has been reached they have largely disappeared from view. Some of the constituents of the infantile sexuality have been deflected into altogether non-sexual channels, *i.e.* sublimated, others have been repressed and survive only in the unconscious mind, a few still find expression in a furtive manner withdrawn from adult view, and the only ones that are allowed to preserve an open existence do so under the condition that an inhibition is interposed between them and their natural goal. The ones last mentioned comprise the affectionate impulses of childhood, the desire to be loved, to receive and bestow suitable caresses, which are permitted and even encouraged by the parents and other authorities. They are characterised by the desire to be loved being far more pronounced than the desire to love, or—what is much the same thing—by the self playing a more important part in the relationship than the object, and, as has been remarked, by the inhibition of any openly sexual goal[1].

The physical changes at puberty, important as they are, do not concern us here, but equally important changes on the psychological side accompany them. They usher in the process which is to end by replacing the type of sexual life we have just described as characteristic of late childhood by that of adult life. When the process is completed, which normally is between the ages of sixteen and eighteen, three things have happened: the impulse is no longer inhibited in regard to its sexual goal; it is directed towards strangers instead of to the parents and persons of the immediate environment as formerly; and the capacity to love has grown stronger at the expense of the desire to be loved, especially in the male sex.

8.

But before these important changes can be brought about, the transitional stage of adolescence has to be passed through, and this is effected in a highly interesting manner. At puberty a regression takes place in the direction of infancy, of the first period of all, and the person lives over again, though on another plane, the development he passed through in the first five years of life. As this correlation between adolescence and infancy is the most distinctive generalization to which I wish to call your attention in this paper, I should like to dwell on it at some little length; it is one to which I would attach considerable importance as affording the key to many of the problems of adolescence. Put it another way, it signifies that the individual *recapitulates and expands* in the second decennium of life the development he passed through during the first five years of life, just as he recapitulates during these first five years the experiences of thousands of years in his ancestry and during the pre-natal period those of millions of years; I believe this to be true of other mental aspects than the purely sexual ones, though it is in the latter sphere that it is most definitely to be observed. It is, of course, obvious that by the word 'recapitulates' one does not mean simply 'repeats,' any more than in speaking of the embryo recapitulating his infra-human ancestry we mean that he literally repeats it. It is just because the process of recapitulation in all three cases is far from being an identical repetition of the former ones that the significance of the features common to the earlier and later stages is not evident and has so long been overlooked. The differences are plainly due to the circumstances in which the development takes place in the two stages being themselves very unlike.

---

[1] This inaccurate phrase is, of course, only a convenient replacement of the clumsier expression: inhibition of the tendency of the impulse to attain an openly sexual goal.

Let us now compare in more detail the sexual development during infancy and adolescence respectively. In both of them five stages are passed through and these are essentially similar in both cases. Any description of them, however, must necessarily be schematic, for one should constantly remember that the richness, complexity and variability in the factors modifying the process of development prevent its being accurately summarised in any simple formula. Nevertheless, when it can be clearly followed it will be found on the whole to contain indications of the five stages in approximately the order given here, though it should again be added that there is never a sharp distinction between the various stages, that they usually overlap extensively, and that they may even all coexist together. After making these qualifying reservations we may enumerate the stages. The first is that of diffuse auto-erotism, in which the various orifices of the body play the chief part. This is followed by the various sub-stages to which the term 'pregenital' has been attached; of these the best studied is the anal-sadistic phase, which is marked by the prominence of the sadistic impulse combined with the numerous physical and mental derivatives of anal-erotism. In the third stage the genital zone proper has achieved a primacy over the others and narcissism is fully established. Closely related to this is the homosexual phase, the distinctness of which varies considerably in different individuals. The final stage is the heterosexual one in which an object chosen from the members of the opposite sex becomes of the greatest interest and the sexual impulse is directed towards her or him respectively.

As was remarked above, these stages are passed through on different planes at the two periods of infancy and adolescence, but in very similar ways in the same individual. The evidence for this statement is derived from actual investigation of the sexual development in particular persons. It is found empirically, for instance, that when a person has special difficulty in passing through the masturbatory phase of adolescence, information obtained about his childhood shows that he had a similarly pronounced difficulty in this respect in the earlier period too. Again, if in adolescence the homosexual trend is especially prominent it is found that the same was true of his early childhood, and so on. It is on the basis of numerous experiences such as these that I venture to propound the general law mentioned above, that adolescence recapitulates infancy and that the precise way in which a given person will pass through the necessary stages of development in adolescence is to a very great extent determined by the form of his infantile development. Some further description is necessary to amplify the schematic account given above of the sexual development during adolescence. That the auto-erotic phase belongs to the earlier stages of adolescence rather than the later is familiar enough knowledge. With it goes the tendency to introversion and a richer life of secret phantasy, together with the greater preoccupation with the self and the varying degrees of shyness and self-consciousness, which are so often prominent features during adolescence. The anal-sadistic phase varies in intensity, but it is characteristic enough for the nice gentle lad of ten to change into the rough and untidy boy of thirteen, to the great distress of his female relatives; extravagance, procrastination, obstinacy, passion for collecting, and other traits of anal-erotic origin often become especially prominent at this age. The narcissism may be shown in either a positive or negative way: the bumptiousness, conceit and cock-sureness of youth are as well-recognised characteristics of this stage in development as are the opposite ones of self-depreciation, uncertainty and lack of confidence; the two sets commonly alternate in the same person. The homosexual phase is more often positive than negative and is far commoner during adolescence than at any later age; that it varies enormously in intensity in different individuals is of course well known. The heterosexual impulse commonly shows the following difference in its fate at the two periods of life. At both it most often breaks through the barriers of prohibition and reaches a directly sexual goal, but whereas in childhood this part of the impulse usually becomes increasingly subordinated to the constituents that are inhibited in respect of this goal, *i.e.* to family affection and the like, in adolescence it is for physiological reasons the directly sexual ones that grow progressively stronger, often entering into severe conflict with the other set before harmony is finally established by a fusion of the two in successful love.

9.

Five features of difference between children and adults have now been mentioned, and I should like to apply to each of them in order the conclusions just reached as to the relation between adolescence and infancy. The first two features, those of intellectual change and greater integration, may be considered together. I remarked that inhibition played a central part in regard to the latter feature, and probably an important one in regard to the former. Now it is surely noteworthy that the two periods of life at which the acquirement of inhibition is most active are those of infancy and adolescence. The lessons learnt in both may differ in their individual form, but they are of a similar order. In its development into childhood the infant learns to hold in suspense the action of various stimuli on the mind (and of course nervous system), to endure it without immediate motor or emotional response, to postpone such response to the most satisfactory moment, and to coordinate it with other forces operative in the mind. The adolescent passing into manhood or womanhood acquires a still higher degree of this capacity to tolerate stimulation and inhibit response. The most typical example from infancy would be the acquirement of control over the acts of excretion, where every feature of the description just given is illustrated. In adolescence a great part of both the natural changes and the training deliberately inculcated may be summed up under the term 'acquirement of self-control,' and it is not necessary for me here to attempt to enumerate, or even to quote from, any of the thousands of examples of this with which you are all familiar. One might perhaps suggest that there is possibly this difference in the process at the two ages, namely, that in infancy greater emphasis seems to be laid on the acquirement of control over volitional motor outflow, while in adolescence it is laid on the acquirement of control over emotional outflow. The child learns to restrain action when it is inexpedient, the adolescent learns to restrain the display of emotion (not only when inexpedient, in this case, but always).

This growth in the power of inhibition at the two ages in question must certainly be correlated with the circumstance that they are the two most emotional ages of man; in fact, it would seem as though the inhibition is a reaction to the rush of emotion so characteristic of these periods of life. A process that is closely connected with inhibition, being indeed in all probability one of its manifestations, is that of 'repression,' and again infancy and adolescence are the two ages at which this takes place on the most extensive scale. How successful it is in regard to infancy is well known. The greater part of the first five years, and always the most important part, is invariably forgotten and remains inaccessible to consciousness (the definition of 'repression'), while commonly enough not a single trace is left of the whole period. In adolescence a second great wave of repression sets in, reaching a height never attained at any other period of life. Ideas which were tolerable enough to consciousness up to puberty, for instance the desire for the pleasure of parental caresses[1], now become repressed for ever, and ideas which later on will once more be freely admitted to consciousness, *e.g.* sensual ones, are often quite banned.

The two characteristics of the third feature were the greater development of altruism, of interest and love for one's fellow-beings and the outer world in general, and the increasing replacement of phantasy by adaptation to reality, the replacement of the pleasure-principle by the reality-principle. Both of these are more plainly manifested later on in adolescence and during the transition from it to adult life, just as in childhood they are related to the transitional period following infancy. It seems to me that they can well be correlated with the change in the sexual life from the manifestations most connected with the self, including auto-erotism, narcissism, and to a great extent even homosexuality (for this can almost be classified as a variety of narcissism), to hereto-erotism, *i.e.* the need for a love-object. This correlation evidently throws light on the first phenomenon, which is largely a matter of extending the libidinal attachment from the self to surrounding objects. The relation of phantasy to reality also comes to be closely connected with the sexual life, although, as Freud has pointed out, it is not originally so. In the competition between the experiences and enjoyments of phantasy and those of real life, one of the great factors that ultimately gives the victory to the latter is the evolution from auto-erotism to hetero-erotism. The accessibility of the source of satisfaction, *i.e.* the self, in the earlier of these phases renders the person largely independent of the outer world, whereas with the development of the hetero-erotic phase the necessity for adaptation to the outer world becomes an increasingly pressing one, and to this must probably be ascribed much of the transition to the reality-principle. In the evolution from autoerotism to manifest hetero-sexuality, phantasy plays the part of a bridge, just as it does in that from forbidden incestuous to permissible object-love.

---

[1] I do not, of course, mean that the acts themselves become intolerable, though even that occasionally happens.

About the fourth feature, that of dependence, much misunderstanding has prevailed. The child has in this respect, as in so many others, been judged from the point of view of the adult. Because the adult sees plainly that the infant cannot subsist without adult care and help, he is apt to assume that the infant must realise this and feel extremely dependent on the adult, being only very gradually weaned from this dependence. In other words, he considers the question of dependence in terms of adult reality and projects the same attitude onto the child's mind. In my opinion, however, this is a very partial view of the state of affairs. The infant's needs are so automatically satisfied, and the distinction he draws between self and outer world is so rudimentary, that he is much more likely to have a sense of power than of helplessness, as we all probably should if our wishes were only gratified with the ease and certainty on which the infant can count. I think that the feeling of dependence, which is only a later feature in the child's development, is not primarily related to the fact that he has to rely on parents for his material welfare, food, clothes, and so on, for this fact he takes very much for granted and it does not produce much psychological reverberation. Psycho-analysis, particularly of cases where the attitude of dependence has been specially pronounced, shows that it is to a far greater extent related to the need for love and that it is a characteristic phenomenon of the hetero-erotic or Oedipus phase of infancy. Normally it diminishes progressively between the ages of six and twelve, but with the onset of puberty it reasserts itself in other forms, among which the familiar displacement of the feelings of adoration and dependence from the Father on to the idea of God is perhaps the most typical. Although this need for personal love to which we refer must psychologically be classed as a part of the sexual instinct, yet the impulses comprising it differ from what are commonly called sexual impulses in being more or less completely inhibited in respect of any sexual goal; the aim is emotional rather than physical union. The regression which takes place during adolescence to the infantile Oedipus relation to the parents is chiefly manifested on the positive side only in relation to the father (with the boy), as hostility and rivalry; the attitude towards the mother at this period is mainly a negative one, a rejecting of caresses and intimacies that up till then had been permitted. At the same time greater activity begins to be shown towards other love objects and the desire to love increases at the expense of the desire to be loved. When the fully adult stage is reached the incestuous attachments are abandoned; a strange love object is found, who is loved not only with feelings of an 'inhibited' nature but also with those of a directly sexual kind. When this process is complete, then adult independence is achieved in all respects. Dependence can therefore be defined as persistent incestuous attachment of the libido and independence as the disposal of it in some other direction. The non-incestuous disposal of it may be either in relation to a love-object or it may take the form of sublimation; most often it is both.

These conclusions, if true, have an important bearing on the theory of education. A child can, of course, educate himself, but by education is usually meant the influencing of a child by another person for certain specific purposes. I would maintain that the essential motive force made use of in this relationship, whether at home or in the school, is the attachment of what we have called 'inhibited libido' of incestuous origin, the same factor that is responsible for the phenomenon of dependence. If the libido is not converted into this form, or evolves out of it, then, other factors being equal, by so much is the child the less educable[1]. Limits are set to education, therefore, in two opposite ways, by auto-erotic functioning or by hetero-erotic and non-incestuous functioning, both of which provide channels for the libido that are not at the service of the teacher. The second of these is of course much the rarer during school life and any sign of it on the part of pupils is vigorously opposed by all teachers. The former, however, is a common enough phenomenon at the school age and is therefore, from one point of view, the greatest foe that the teacher has to encounter, which is the reason why teachers denounce it in unmeasured terms as the unpardonable sin. That their efforts to deal with it meet with very limited success is well known, and this must in great part be ascribed to the fact that they refrain from making any study of the nature and significance of the phenomenon.

---

[1] From this simple statement it is not to be inferred that manifest sexuality (either auto- or hetero-erotic) and educability are purely reciprocal functions. The matter is complicated by the circumstance that excessive repression, carrying with it complete suppression of all forms of manifest sexuality, is very apt to involve also the 'inhibited libido' on which educability depends. Indeed, I should be inclined to ascribe the notorious fact that children are usually much more 'troublesome' and 'difficult' from twelve to fifteen than from eight to eleven to repression of their 'inhibited libido' at the former age rather than to the struggle for manifest sexuality, with the revolt this entails. Incidentally, I might add that the present law and custom of regarding the period from five or six to fourteen as an educational unit is not at all in accord with the conclusions of clinical psychology, which suggest that the period from five or six to twelve is not comparable with that from twelve to eighteen and cannot profitably be dealt with on the same educational lines.

It will thus be seen that excessive repression and imperfect repression equally diminish the capacity to be educated. Future researches of a more exact kind will doubtless be able to establish an optimum between these two extremes, one at which educability will be at the highest point attainable by each particular child.

Something should be said here, if only a few words, on the important subject of the various forces acting on the adolescent in connexion with the process of growing up. These are both external and internal, the latter being probably the more important. Each set also comprises two groups of forces, acting in the direction of growing up and against this respectively, so that there are four groups in all, two favouring and two hindering the growing up, two internal and two external. On the one side there is the vital unfolding of the personality, recapitulating the history of the species, which manifests itself even in early childhood in the form of ambitious phantasies and performances. This is the strongest force of all and normally it prevails; it is aided both by the disinterested affection of the parents, which recognise what is to the child's ultimate advantage, and by their ambitiousness operating through the mechanism of self-identification. On the other side there is the conservative tendency to cling to former phases of development and the reluctance to exchange these for new and unknown ones, at all events until there is some assurance that the pleasures surrendered will be replaced by fresh equivalents. This tendency, which includes what Freud indicates under the terms 'regression' and 'fixation' as well as other processes as yet insufficiently studied, is certainly far stronger than is commonly supposed. It is powerfully reinforced by pressure brought to bear in the same direction by parental influence, perhaps more from the side of the mother. The parental motives hindering the growing up of the child are in the majority of cases stronger, and often much stronger, than those favouring this development. Their two chief constituents are jealous rivalry and the reluctance to give up the pleasure previously derived from the children, both of course being usually denied by the parent. The pleasure that the parent obtains from his children, and more especially from her children, is often in inverse proportion to the age of the children, and, furthermore, the renunciation of it commonly betokens a loss rather than an exchange. The upshot of the whole situation is that in most cases growing up is effected by an internal 'urge' on the part of the child or adolescent against an almost equally strong internal resistance and also against serious external opposition. Little wonder that successful accomplishment of the process belongs to the rarities of human existence. Throughout the years of adult life the fight continues of freeing the personality from the restrictions that operate to excess during childhood and adolescence, until this evolutionary impulse to grow up is met by the counter-force of the involutionary process which conducts us, through 'second childhood[1]' to the grave.

10.

The foregoing considerations raise from a new point of view the problem of the biological significance of puberty. In the majority of animals puberty, in the sense of capacity to reproduce the species, *i.e.* sexual maturity, takes place subsequent to birth, at a point the distance of which from it varies enormously in different species. The pre-puberty period is, I think, longer in man than with any other animal, constituting some 20 per cent. of the total duration of life. The strongest attempts are made on the part of society to extend it until it covers as well the most active years of sexual life, those from fifteen to thirty-five. Whether this is a biologically harmful tendency, one due to purely sociological and psychological factors, would make an interesting question for discussion. More germane, however, to the present topic is the consideration that man appears, so far at least as my knowledge goes, to be the only animal in which the earliest manifestations of sexual life do not proceed smoothly up to the stage of full maturity. In man, on the contrary, as was pointed out above, sexual development has to be gone through twice over, with a more or less complete break, Freud's so-called 'latent period,' in between. Instead of the development progressing evenly and culminating in puberty, what happens is that a rehearsal of the whole process is performed in the first years of life and then on reaching puberty the individual has to start again at the beginning and go through the drama in a more serious and sometimes tragic fashion.

In conclusion I would say that I am particularly aware of the sketchiness of the outlines I have drawn in this paper. In the endeavour to deal with such a vast and important field I have been obliged to express many points in such a summary and schematic way as to deprive the statements made of the accuracy which could only be attained by introducing the modifications and qualifications impossible in a short exposition.

---

[1] If the views here expressed are correct, this should more properly be termed the 'third childhood.'

A VINDICATION OF THE RESONANCE HYPOTHESIS OF AUDITION. IV.

BY C. R. G. COSENS AND H. HARTRIDGE.

(*From the Cambridge Physiological Laboratory.*)

(1) *Experimental evidence is given in support of the statement that change of phase affects the quality of a musical chord if its constituent tones are accompanied by harmonics.*

(2) *With pure tones, free from harmonics, change of phase does not audibly affect the quality of the mixed tone.*

(3) *This fact is advanced as being very strong evidence that harmonic analysis takes place in the ear, such as would be performed by resonators.*

IT is well known that if two simple harmonic motions of different rates be combined, considerable alteration in the combined motion is produced by altering the phases. Calculation shows that a pair of tones in the ratio of 3 to 1 give larger changes in wave form with change of phase, than other ratios. Thus in one relative phase the sound wave has a single sharp peak, whereas in another it exhibits two peaks with a trough between them. Tones in this ratio of 3 to 1 are therefore ideal for ascertaining whether the ear is affected by a change in the actual shape of the sound-wave curve brought about by change of phase.

Now if the ear in transmitting to the brain the nervous impulses arising from two or more simultaneous tones is uninfluenced by the phases of these tones while it reacts to their individual frequencies, then it would appear certain that there must be some mechanism in the ear which is capable of performing a pure harmonic analysis of the air motion reaching the ear. A study of tide predictors and the like will show that the only physical machine capable of performing such a pure harmonic analysis is a system of resonators. By ascertaining, therefore, whether the ear can or cannot detect a change in the relative phases of the incoming vibrations an additional test to those previously described can be applied to ascertain whether there are or are not resonators in the internal ear.

With regard to previous work on the effects of phase difference König supported Seebeck in the view that phase affects the quality of a compound tone, by experiments with his wave syren. His experiments depended for their basis on the truth of a number of postulates, some of which are:

(1) That the amplitude of the compound sound-wave set up in the air is proportional to the volume of air passing out of the nozzle of the syren at any instant.

(2) That this volume of air is proportional to the longitudinal dimensions of the nozzle of the syren at any instant.

(3) That the serrated metal edge of the syren disc represents the amplitudes and frequencies of the wave-forms with absolute accuracy.

There would appear to be very good reasons for doubting whether any of the above postulates are true, thus Ter Kuile[1] has pointed out at least one weak feature.

Helmholtz held the opinion, contrary to that of König, that phase had little or no effect on quality and in this view he was supported by the experiments of Hermann[2].

Ter Kuile stated that he observed definite changes in the character of a musical cord in which slow beats had been produced by mistuning one of the tones. Such changes are almost certainly due to interference between different elements (primaries, partials or combination tones) which happen to be of the same pitch. The following experiment, which we have carried out, may be cited in support of this explanation.

A Helmholtz syren with two wind chests and syren discs was arranged to produce two notes, one on each disc, whose frequencies were in the ratio of 1 and 3. On slowly rotating the upper wind chest when the syren was in action so as to make one of the notes sharper or flatter than the other beats were produced, the number of which per second increased with increased rate of rotation of the wind chest. Thus, 18 beats per second were produced when the wind chest was rotated once per second, 36 when it was rotated twice and so on. It was found further that there were 18 equidistant positions at which the sound produced by the syren had a weak upper tone and 18 intermediate positions at which the upper tone was strong. In fact the beats were produced by inter-action between the higher note and that harmonic of the lower note which had three times its frequency. In certain positions of the wind chests the wave motions summed, giving a strong upper note, and in other positions they cancelled, giving a weak note. This experiment shows, therefore, not only that the tones produced by the syren are impure, but also that with such impure tones change in phase ought to produce recognisable changes in quality.

---

[1] *Arch. f. d. ges. Physiol.* LXXXIX, 347.

[2] *Ibid.* LVI, 470.

In the absence of any conclusive proof that change of phase does or does not affect the quality of two pure tones reaching the ear at the same time, it seemed to us advisable to try to obtain fresh experimental evidence. In seeking for two suitable sources for this purpose the possibility of using two pure tone syrens[1] presented itself. It seemed, however, that the mathematical calculations on which the form of the syren and jet orifices had been computed had neglected the following facts:

(*a*) that air has momentum;

(*b*) that the quantity of air passing the orifice is not proportional to the opening, since when the area of the opening is small 'wire-drawing' takes place;

(*c*) that when the orifice is closed, partial compression of the air occurs with a subsequent excessive rate of flow of air, when the orifice is again uncovered.

It seemed to us that the tone of this syren might be less pure therefore than has been thought. This idea was confirmed by the knowledge that experiments performed elsewhere had demonstrated the presence of harmonics in the note from such a syren. We therefore employed tuning forks, both of them electrically driven, one of 426 vibrations per second, the other being adjusted to give 142 vibrations per second[2], *i.e.* in the ratio of 3 to 1 because this ratio is found to give the largest change in wave form with change in phase as explained above.

These were arranged on separate electric circuits so that there was no possibility of the phase of one being altered by oscillations produced by the other in the electric circuit. With the upper tone sounding continuously, the lower tone was alternately started and stopped at irregular intervals so that its phase should have a random relationship to that of the other fork. This experiment was repeated a number of times without our being able to detect any change in quality in the mixed tone produced. It would seem then that with pure tones, change of phase does not affect quality even in so favourable a case as tones in the ratio of 1 and 3.

---

[1] Milne and Fowler, *Proc. Roy. Soc.* A, 1921, xcviii, 414.

[2] We wish to thank Dr Myers for the loan of the tuning forks and the syren used in these experiments.

Presumably, therefore, a pure harmonic analysis of sound wave-motion occurs in the ear, and therefore there must be a suitable mechanism for rapidly effecting such an analysis. The only mechanism known to science with the required physical properties is a series of resonators. We must conclude, therefore, that there are resonators in the internal ear.

(*Manuscript received* 20 *November*, 1921.)

INDIVIDUAL DIFFERENCES IN LISTENING TO MUSIC

By CHARLES S. MYERS.

(*From the Cambridge Psychological Laboratory.*)

1. *Plan of the Investigation* (pp. 52–54).
2. *Comparison with the results of the writer's previous investigation* (pp. 54–56).
3. *Comparison with the results of Bullough's previous investigation* (pp. 56–58).
4. *The objective aspect in the technician: His suppression of other aspects* (pp. 58–60).
5. *The absence of associations in the most unmusical* (pp. 60, 61).
6. *The occurrence of associations among the musical* (pp. 61–63).
7. *The relation of the character to the intra-subjective aspect* (pp. 63, 64).
8. *Symbolization of the art material* (pp. 64–66).
9. *The aesthetic value of the pragmatic and objective aspects* (pp. 66, 67).
10. *The aesthetic value of the intra-subjective aspect* (pp. 67, 68).
11. *The aesthetic value of the meaning of music* (pp. 68–70).
12. *The importance of 'distance'* (pp. 70, 71).
13. *The importance of the 'mystic' feeling* (p. 71).

1. Plan of the Investigation.

In 1914[1] I published the results of an inquiry into the individual differences of aspect adopted in listening to the *tones* of tuning forks, simple and combined, presented singly and in pairs. This inquiry was planned as complementary to the previous work by Bullough[2] on individual differences in the appreciative aspect towards *colours* and *colour* combinations; and it was intended to be introductory to the study of individual differences in listening to *music*, which is attempted in the following paper.

This study is based on the investigation of fifteen subjects, nine men and six women, all University graduates or their wives, eight of whom had also been my subjects during the simpler experiments with tones just mentioned. These fifteen persons were in various degrees musical. Two of them, J. and M., were highly gifted professional musicians; three, B., F. and G., though not averse to music, may be regarded as relatively unmusical. Between these extremes are to be ranged three subjects, H., K. and N., who were fairly accomplished amateur musicians; five, C., E., L., O. and P., who, though unable to perform music, were of extremely artistic temperament; and two, A. and D., who may be described as having an average degree of musical taste. One of my subjects, E., was a Japanese.

---

[1] *This Journal*, 1914, VII, 68–111.

[2] *Ibid.* 1908, II, 406–463; 1910, III, 406–447.

In order to secure uniform presentation of the material, I employed the gramophone for the production of the music, using the best available records and instrument for the purpose. Most of my subjects found little difficulty in dismissing from their minds the artificial conditions of the experiment. They listened to the music as they would have listened to it in a concert hall. Indeed, one subject, C., of extremely artistic temperament though without musical training, remarked that the conditions were "ideal" for listening, as the comfort was greater than at a concert and there were "no aggravating people and no worrying illumination!" The music consisted of Beethoven's Overture to *Egmont* (Op. 84), Tschaikowsky's Valse des Fleurs from his Casse Noisette Suite (Op. 71 *a*) and his Italian Capriccio (Op. 45), Mendelssohn's Overture to the *Hebrides* (Fingal's Cave) (Op. 26), the first of Grieg's Symphonic Dances (Op. 64), and Kreisler's setting and playing of Couperin's Aubade Provençale—all, save the last, being orchestral performances.

Each subject listened to two or three of these six records at a single sitting. More than one subject never listened on any one occasion. Most of the subjects gave me two sittings, so that the average number of records to which each of them listened was between four and five. Before a subject first listened at any sitting, he was always initially given a record to hear (not one of the above six), in order to become accustomed to the experimental conditions. He was seated in a comfortable armchair, with his back to the gramophone. The first time he heard any one of the six records, he was left absolutely free to relate his impressions and his attitudes at the close of it. He was then given a pencil and paper and allowed to hear it a second time, noting down any further impressions in a mnemonic form, so that he would not fail to communicate them to me at the close of the second hearing and could describe how that hearing differed from the first. By a simple arrangement of a travelling index and a fixed scale which I attached to the instrument, any required part of the piece could be reproduced from the record at will. With the help of this contrivance, certain selected short passages were finally presented to the subject and he was asked to record his impressions of them.

2. COMPARISON WITH THE RESULTS OF THE WRITER'S PREVIOUS INVESTIGATION.

In all those subjects who had previously submitted to introspection in the tuning-fork experiments, it was easy to recognise broadly the same aspects as they had there displayed when they came to listen to the more complex material of a musical work. That is to say, the material might appeal to them (i) for the sensory, emotional or conative experience which it aroused, (ii) for the associations which it suggested, (iii) for its use or value considered as an object, or (iv) for its character personified as a subject. These four aspects, which, following in the main Bullough's valuable work[1], I had distinguished respectively as (i) the intra-subjective, (ii) the associative, (iii) the objective, and (iv) the character, were readily recognisable, more or less according to the prominence they had exhibited in the introductory, more elementary, tonal experiments. Thus one subject, A., of average musical taste, who, when listening to tones, had proved predominantly of the associative type, recorded such associations as the following when listening to the music: "I was in the Queen's Hall, a fair girl in a pink dress was playing and another girl was accompanying her. The violinist had a sad look about her. I felt she had had a sorrow in her life." "I saw F— playing it as a duet as he used to do." "It started with a stage full of people—a tremendous lot of movement about it and brightness. The people were all in costumes. Then a singer came on from a house on the right side of the stage, telling a pathetic love-story. Then I lost the solo part, the stage became dark, and all the people left, except, I believe, the singer who remained in some quiet corner." "It reminds me of Ely Cathedral." Compare these with the subject's previous replies when a series of pure tones was given her: "I saw a woman in evening dress as if she were singing." "I saw a certain room with F— at the instrument just starting to play with his right hand." "I think I saw the inside of a church." Or take another subject, G., relatively unmusical, who, when confronted with single tones had always shown a keen conative tendency—a strong desire to do something with them, *e.g.*: "I tried to connect it with piano sounds but couldn't." "I can't join it on to piano sounds: I try to imagine it in its place in a piece played by a pianola." "It made me wonder whether it was a sound that could come into a tune." "I considered what was the difference between this and the last." He is always trying to give the tone utilitarian value. When listening to the gramophone records, he proved to be one of the few who were preoccupied with the drawbacks of the instrument. "I don't like it. It's the fault of the gramophone, its tinny sound....Then I liked it, but my mind was much occupied trying to pick out the instruments of the orchestra. It seemed to me as if certain instruments felt the ill-effects of the gramophone...more than others." "I did not seem able to keep up with the more excited mood, and I had a feeling of disappointment at this inability." Another subject, C., who, in the case of the tuning-fork experiments, had referred frequently to the "movement" which the sound underwent when his attention was directed from one tone to the next, constantly alluded to the "patterns" formed by the music in the present experiments. "It falls into a pattern." "It is the sequence of sounds of different pitch that makes the pattern." "Then the pattern changed." "Then came a pattern of different type."

---

[1] My main departures from Bullough's scheme consisted in (i) the inclusion of conative experience in his 'physiological' aspect which I termed the 'intra-subjective' (a term since adopted by Bullough), and (ii) the extension of his 'objective' aspect to include the consideration of the material not only in relation to the subject's standard of purity, pitch, etc., but also in regard to its use and meaning generally as an object. In this paper, I propose to separate the last-named extension and to designate it the 'pragmatic' aspect.

Here is an example of a subject, D., who, in the earlier experiments with tuning-fork tones had shown herself to belong to the physiological sub-group of the intra-subjective aspect, returning such answers as: "I felt a touch on the tympanum"; "I felt a stinging up the right arm, as if the first finger touched a copper spring that rebounded"; "I felt warm in the ear"; "I had a lazy feeling." I give some extracts from her reports on the gramophone pieces: "A restful feeling throughout..., like one of going down stream while swimming....I wanted to throw myself back and be carried along." "During the dance movement I imagined a gentle breeze and felt diaphanous things floating in the wind....The breeze came in contact with my right cheek." "Suddenly it seemed as if the orchestra was in a pavilion in a garden, which then revived all kinds of organic sensations—walking about in the garden, feeling warm. Then I realised that I had been warm before." "My two ears seemed joined together, drawn to one another by something elastic." "A circular movement. I was being turned round very slowly." "An echo heard after each chord, with definite vibration inside the right ear." "A very definite olfactory sensation of open air and grass." In the tuning-fork experiments this subject had shown tendencies also to the character type and to visual symbolism, describing, for example, a given sound "as purple and maroon, which are like the lights one feels rather than sees, as in shutting the eyes in a bright light or in pressing on the closed eyelids. There is no outline or demarcation between the purple and maroon. They seem to fill the entire field[1]." So, too, in listening to music, she reports: "Flourishes, flamboyant architecture, suggested." "A ball raised by something elastic and falling back by its own weight: this happened three or four times." "The piece seemed represented by long and short dashes. No sense of extent, no feeling of areas—only contrast by dashes."

It is, I think, of no little psychological and experimental interest to note the consequent value of investigations with the simplest materials for our understanding of the aspects adopted, the kind of appeal made, in the case of works of art. It seems probable that the experience of beauty is rooted in man's remote past when it could be evoked by such simple material as one or two tones or splashes of colour, *i.e.* by the most primitive *forms* conceivable of art material, just as to-day it is evoked by more complex forms. As Bullough well remarks[2], the more abstract material evokes an aesthetic experience in miniature. But it would be absurd to expect too close a correspondence between the attitudes of listening to tones and listening to music, for reasons to which I shall allude presently (page 57).

3. Comparison with the Results of Bullough's previous Investigation.

As I stated in my previous paper[3], there are exceptions to the general correspondence between the aspects of listening to tones and of regarding colours. In the case of the subject A., for example, whom I have already described as having proved strongly of the associative type for tones and music, "a colour is almost a human being and is endowed with such personal attributes as morose, cheerful, insincere, serious, playful, etc." When, however, she comes to listen to music, this character aspect, though by no means abolished, is largely replaced by the intra-subjective aspect, *i.e.* the sensory effects, the changes in feeling, the experiences of self-activity obtained from the music. She reports: "That was lovely.... Something lifting, raising you inside. Like what one gets in church." "I imagine I am going to die, as if life were just ebbing out." "A great feeling of happiness, followed by expansion inside, leading to great excitement and breathlessness for a moment." Nevertheless, the character aspect may be detected, *e.g.* "Very very beautiful, but very mournful and sad: a drawing out of the agony." "It tried to be light-hearted, but was all the time very sad." "The first two bars are like feelings of anticipation, then that lovely feeling of depth and goodness coming out of you that you get in church. Yet all through, very very sad. Sensations of something coming up from the abdomen and surging up to the head." "I cannot get anything out of it but depression....A delightful feeling of welcoming the end....I had feelings of sorrow and dissatisfaction with everything. They gained on me. All the time I was trying to get the better of those feelings, but they wouldn't leave me." We see here how the character aspect may be inhibited and replaced by the intra-subjective aspect, *i.e.* by the strong feelings to which in this subject the music gives rise.

---

[1] *Op. cit.* 84, 85.

[2] *This Journal*, 1910, III, 447.

[3] *Op. cit.* 86–90.

Comparing Bullough's experiments with colours and mine with tuning-fork tones, I found that in the latter purity of type was far rarer. I seldom met with a subject whose replies were consistently given from a single aspect. This I attributed to our different procedure. "Bullough was occupied solely with the determination of that aspect or attitude which evoked acceptance or rejection of the colour. His object was to find out why a colour was disliked," whereas it was my purpose "to investigate the entire and spontaneous attitude of the subject towards musical sounds and not merely to ascertain the attitude which he would adopt in order to pronounce an aesthetic judgment upon them[1]."

In the case of the musical experiments described in this paper, the multiplicity of attitudes increases still further. Hardly any subject shows himself absolutely pure to one type, to the complete exclusion of others. The material here is far more complex, comprising no longer merely pure tones, but also melody, rhythm, tone colour, polyphony, harmony, etc. Even in my simple experiments with tuning-fork tones, I had found indications that, whereas the liking or disliking of a single tone was determined more often by sensual changes, in the case of two simultaneous tones it was determined rather by emotional and conative changes[2]. Moreover—unlike a tone, a colour or a picture—a musical piece is not presented all at once as a whole, but is unfolded gradually, like a poem or drama. Such differences in complexity and in mode of presentation will naturally result in a many-sided appeal to the subject's possible aspects, now inviting one, now another, now a third, whereas, in the case of the more abstract, simpler material and with less natural, more restricted methods of procedure, fewer aspects are appealed to, and of these only the more habitual and the more ready.

---

[1] *Op. cit.* 86.

[2] *Ibid.* 95, 96.

From these considerations we should expect to find that different kinds of music may evoke different aspects in the same listener, so that he comes to realise that there are many different ways in which he can appreciate music. As one of my subjects, K., remarked, "Sometimes I listen to music seeing the orchestra and attending to the *technique*, sometimes enjoying visions of forests, etc., that come before me, sometimes paying regard to the meaning, the sadness, etc., of the piece."

An artificial purity of type may arise from the process, conscious or unconscious, of inhibition. We have seen, for example (p. 56), how the character aspect may be inhibited by the intra-subjective aspect: instead of regarding the music as a personality, the listener's attention is directed to the sensations, emotions and impulses evoked in him. Here a 'higher' aspect is replaced by a 'lower' (for aesthetically the character aspect stands unquestionably higher than the intra-subjective[1]). But the converse may also occur. Instead of the lower aspect being released, it may be controlled by higher inhibition. One of my subjects, N., observed: "I should have felt distinctly wretched....but I very rarely let myself go." Such higher control is especially exercised over the associative aspect. "I object to these suggestions (*i.e.* associations)," says another, D., "for I find then that the music...is not listened to for itself. I want to do that." "If I had not had to give introspective data," says yet another, H., "probably the images would have passed unnoticed." (Cf. also p. 60.)

4. THE OBJECTIVE ASPECT IN THE TECHNICIAN.

It is interesting to find that the objective aspect in which the musical material is considered in reference to the listener's standard, occurs most frequently among those technically trained in music, who tend to adopt a critical attitude and are interested in the material of their art. Consider, for example, the reports of the professional musical subject, M.: "I noticed the second horn was too loud....When the second tune came with the 'cellos, it didn't stand out enough." "I noticed by what simple means in these modern days he gets his effects. I noticed also...how he gathered up his climax by syncopation." "As always in Beethoven, one must notice the tremendous...contrasts, especially dynamic contrasts. His crescendoes always give me pleasure. Beethoven makes scale passages so much more interesting than, say, Liszt." "As usual, the violinist uses too much *vibrato*....The sweep up the strings made me feel quite sick." This subject remarked: "I now nearly always view music from the critical standpoint. I conduct: I compose. I always want to know how the conductor is getting effects if it is a new work, and what will be his rendering, if it is an old one....I never think of 'programme' unless it is suggested to me....To me music is never sad or joyful. I only get aesthetic impression."

---

[1] Cf. Bullough, *This Journal*, 1910, III, 417.

This highly musical subject believes, then, that in listening to music he suppresses not merely the more lowly intra-subjective and associative aspects, not merely all personal feelings, activities and imaginations, but also the character aspect, in favour of the objective aspect, the critical, analytical standpoint. Now it is interesting to find that this very subject, when he was presented in my earlier experiments with tuning-fork tones, had shown very distinctly the character aspect, personalising tones as 'trivial,' 'grim,' 'mysterious,' 'stupid,' 'silly,' 'bony,' 'bare.' Even in the present musical experiments, a tendency towards the same aspect is revealed. "The cadenzas," he remarks, "are rather vulgar and horrid." "That is simply a piece of empty pomposity, holding you up to expect something beautiful, and then you get *that*." "The introductory solo accompaniment...is in the last degree trivial."

Upon the release of the character aspect even the associative aspect occasionally escapes in this subject from its inhibition. Thus the trivial character of the music suggests to him the stage. "I saw the orchestra and footlights." The same escape of associations had occurred when he was listening to the tuning-fork tones; a 'stupid' note suggesting 'a stupid parrot,' and a 'materialistic' tone suggesting 'a successful tradesman.' These associations seemed to be determined exclusively by recent experience. Thus the stupid parrot was connected with a recent visit to the 'Zoo.' The stage music, just mentioned, "carried me back to the time I heard the opening of Rimsky-Korsakow's opera, *Ivan the Terrible*, last night." So, too, his recognition of the *Hebrides* Overture brought up visual images of "a cave, rocks, sea-waves...a sea-serpent poking its head out of the cave [suggested by the trombones], dancing spray, with the sun on it. I could draw the exact picture. I have been reading lately about Hebridean folk songs."

The occurrence of these associations filled M. with amazement. "It is not like me at all," he protests. On another occasion he observes, "I opened this with a dog fight....The opening of the second part was a dance of savages (this is *amazing* to me). I could see the red and blue round the loin cloths. Then, I think, I pulled myself together." In other words, he checked or inhibited all tendency to association, which under these experimental conditions was specially favoured. Another subject, D., emphatically of the intra-subjective type, similarly remarks (cf. p. 58): "I always try and banish all imagination when listening to music." What a contrast this presents to the highly artistic, but musically untrained subject, P., who insists, "Music always gives me the sight of so many charming things. That's why I like listening to it."

Trivial, unreal or meretricious music is specially liable to evoke in imagination stage scenery, the subject regarding the feelings or actions of the persons imaged from a distance without sharing himself in them. "I was up in the theatre," reports K., "looking down." "I," reports E., "felt no deep emotion. But there was much emotion in the soldiers." "The beginning," says L., "reminded me of a stage, people coming on. It was trivial, theatrical." Observe now the contrast: "Then it passed to out-of-doors, real not stage-like, in a wood, with sunlight, a vast procession of people slowly moving...with gold-coloured dresses, some green, all brilliant."

5. The Absence of Associations in the most Unmusical.

It would be indeed surprising if the *tendency* to associations were really lacking in the most musical persons. For in all the rest of my subjects associations are present in a varying degree, save in C., who is ever occupied with his 'patterns,' and in F. and G., the two most unmusical. Both the two latter adopt predominantly the intra-subjective aspect (cf. p. 54). F. allows the sounds to act physiologically upon him and it is only as a *pis aller* that he advances reasons for his preferences from the objective aspect. He does not "trouble about the meaning" of music, probably because it has none consciously for him (musically or otherwise). He never attends concerts. He hardly knows one tune from another. The following are samples of his reports: "It's just the kind of thing I like. But...why I like it, I can't see....I didn't like the beginning when the high notes came. I never can stand high-pitched notes." [Here he is driven to the objective aspect.] "I always like a thing of low intensity, gentle: that, I feel, is a much better kind of thing than a good crash of sound....I was soothed almost as if asleep.... Very charming indeed. Just the kind of thing I like....I can't think of any meaning of the slow and the quick themes....I shouldn't trouble about the meaning." Despite the fact that he is extremely unmusical and gets so little aesthetic enjoyment from music, F. shows almost invariably correct taste in his judgment of good and bad music.

The absence of associations in my two most unmusical subjects is of undoubted interest in regard to the origin and fundamental basis of musical enjoyment. The influence of sexual experience and of juvenile imagination is shown in the fact that the commonest figures evoked in the recorded associations were those of lovers, dancers, soldiers, villagers, savages, fairies, fauns, and goblins. Associations of a sexual character occurred in eight of my fifteen subjects (C., E., F., H., J., K., L., O.) and associations of dancing in ten (A., D., E., H., J., K., L., M., O., P.). Six subjects (A., B., J., K., M., P.) reported associations in which a stage full of moving people was presented; six (D., H., E., L., O., P.) in which the scene was laid in the open air; and five (A., B., D., M., P.) in which the orchestra, the conductor or a musical instrument appeared. Three subjects (A., H., K.) imagined themselves in a concert hall: three (A., H., O.) in church.

There can be no doubt that such associations were often responsible for the aesthetic enjoyment of the music in these experiments. Indeed, the close biological relation of the origin of music to sexual display and to movements of the body in dancing would have made this conclusion *a priori* probable. In the grossly unmusical music evokes no associations, because it evokes no corresponding emotion. In the professional musician, music also evokes few or no associations, because he tends to inhibit them by his assumption of a critical, objective attitude. Among the most highly musical, associations tend also to be repressed, because the music comes to be listened to for its own meaning and beauty, apart from the meaning and beauty derived from associations. In four of my five subjects whose temperament was extremely artistic but who had little or no technical knowledge of music, associations were to a large extent replaced by symbols, *e.g.* of pattern, colour, expanse, the activities of which, however, tended themselves to evoke associations.

6. THE OCCURRENCE OF ASSOCIATIONS AMONG THE MUSICAL.

When the average person listens to music, then, associations are enjoyed for their own sake, adding enormously to the total aesthetic appreciation obtainable. The associations may be in themselves beautiful: they invite the listener to share in the beauty of a story and in the emotions of the persons created in his imagination. Among the more highly musical I find that associations are more particularly apt to intrude when the music is felt to be 'stagey,' unreal, meretricious, or vulgar. Thus M. (cf. pp. 58–60) reports associations as the music "began to get more barbaric" and as he "lost interest in the music." He observes, however, "The middle of the second movement [which he enjoyed] switched me off my imagery, and I returned to the pure consideration of the music."

It is by no means strange that associations should appear among the highly musical when music lacks interest or inherent beauty, whereas the less musical tend to appreciate music not so much on the grounds of its inherent beauty as for the enjoyment of the associations evoked. The explanation depends on difference of aesthetic level, the level of the musically gifted person standing higher than that of one averagely musical. So long as the former, attending merely to the music, *qua* music, can maintain his high level of aesthetic enjoyment, associations are debarred from consciousness. But when, for any reason, he fails to maintain that level, *e.g.* because his aesthetic appreciation ceases, then the products of lower-level aspects enter, *e.g.* associations more or less incongruous with the enjoyment of beauty.

In order that associations may be enjoyed for their beauty, either the music must be wholly neglected, and the story, the imagery, the wealth of colour enjoyed as if it were a work of art—which is seldom possible, or the associations must blend or 'fuse' in their general meaning (on which their beauty depends) with that of the music. Otherwise they can have no aesthetic value, but are merely affectively toned with pleasure or displeasure, or at most excite in the listener feelings of joy or distress, according to their cognitive or emotional content. In the following report from one of my subjects, D., we see the distinction between unfused associations and associations of actively aesthetic value which fuse with that of the music: "I object to these suggestions, for I find that the music...is not listened to for itself. But," she continues, "when the suggestions and the music absolutely blend, there is the completest and greatest enjoyment, greater than when there is music alone. They won't blend here, because the dramatic scene will go on quite well independently of the music."

To quote another occasion of this lack of fusion: "It produced," says D., "the idea of someone trying to be persuasive. I wanted to know how the persuasiveness would go on...if it would succeed....It seemed to be a dramatic development without any images. This dramatic is quite distinct from the musical development. They run parallel. There are two people concerned in the dramatic development—the persuader and the persuaded....There is no response to the persuasion: it is a failure. The characters disappear; and the music behaves like a Greek chorus, going over what has occurred in a philosophical manner." Fusion appears to be here lacking between the wordless meaning of the music and the meaning of the imageless thoughts it evoked.

7. THE RELATION OF THE CHARACTER TO THE INTRA-SUBJECTIVE ASPECT.

Bullough places the character aspect at the highest level in regard to aesthetic enjoyment; and he is unquestionably correct in according to it a higher rank than the intra-subjective (his physiological) aspect. But, as in my previous experiments with tuning-fork tones, I find it difficult to accept his view that the character aspect is *derived* from the intra-subjective. As I observed in my previous paper[1], "such characters as 'jolly,' or 'high spirited,' may be ascribed to a sound or a colour even under conditions when he himself feels sad or depressed." As a further proof of the independence of the 'characterization' of music and of the intra-subjective experience to which the music gives rise, the following reports may be quoted. A.: "The piece sounded cheerful in certain parts. But I felt in a contrary grain all the time." N.: "It's all so intensely sad. All the time I was wondering whether it was cheap or not. I came to the conclusion that I ought to be moved. I *was* much moved by it after this conclusion. It was quite upsetting; it made me feel sad. I still see the long funeral procession." That is to say, the sadness of the listener was secondary to the sadness characterized in the music. So, too, another subject remarked: "I noticed first the mournfulness of the music and then its effect on me." Or N.: "A distinctly pathetic ring about it. I should have felt distinctly wretched, if I had got regularly into it, but I keep myself from this at a concert. I very rarely let myself go." Or J.: "There was a note of sadness among the dancers in parts, a sort of regretfulness. I think this sadness affected me secondarily to the stage sadness." Or H.: "The music seemed as if it were joking with me: it made me want to smile."

We see then that the art material may be personalized and characterized as morbid, jovial, insincere, dainty, mystic, reckless, playful, etc. without necessarily having *previously* evoked or *consequently* evoking morbid, jovial, etc. feelings in the listener. The varying degree in which the subject may identify himself with the character ascribed by him to the music, is well illustrated by the following reports. H.: "I felt the yearning character of the first *motif*—a sense of tears in it—which was partly in the *motif* and partly in me. It reminded me of a time when I actually wept in listening to an intensely beautiful chorus, so beautiful that it hurt." O.: "It has expressions rather in the way that a face has expressions. I didn't see a face....I almost personified the music as having expression, a shade of feeling being implied." "There is something sinister about it. It gave me a feeling that made me appreciate how sinister it was."

---

[1] *Op. cit.* 79.

We have to remember that a tendency to personify inanimate objects is extremely primitive and deep-rooted. As Stout remarks[1]: "The cataract or the whirlpool appears a living thing to the poet in his poetic moods: for in these moods he ignores the fact that the water is behaving in accordance with certain abstract laws under certain given conditions. This fact is not *ignored* by the savage: it has never been realised by him. Hence what may be called a transient play of imagination in the civilised mind is the permanent and serious attitude of the savage mind." If Stout had at his own disposal a more strongly developed character aspect, he would realise that it is the persistence of this so-called mark of the savage that leads to the aesthetic personalization of colours, tones and music.

Now it is evident that for aesthetic enjoyment to be perfect, there must be no conflict between the feelings of the listener himself and those inherent in the character of the music. Nor must there be conflict between the character of the music and that of the persons or the story which the listener's imagination may call forth. The need for harmony between these three factors—the feelings of the listener, those inherent in the "character" of the music, and those of the persons in the story imagined—is well illustrated in the following report from one of my subjects. O.: "I almost personified the music,...a shade of feeling being implied. I felt that I had had that feeling before and could sympathize with what the chap was trying to explain." Such sympathy and understanding must add as much to the conditions favouring aesthetic appreciation as antipathy and ignorance must militate against them. *E.g.* A.: "The piece sounded cheerful in certain parts. But I felt in a contrary grain all the time." Or: "I don't know what the end was...I couldn't fit it into my lines of thought." Or G.: "I couldn't keep up with the more excited mood and was hence disappointed."

8. SYMBOLIZATION OF THE ART MATERIAL.

In some persons, as we have seen, instead of the music being endowed with the characters of a human personality, it is symbolized in material form, *e.g.* by dashes, prisms, cylinders, circles and patterns. D.: "The piece seemed represented by long and short dashes. No sense of extent, no feeling of areas—only contrast by dashes." C.: "A frightfully interesting medley of sounds from the pattern point of view." "The pattern is perhaps most like those of glass marbles. It is in three dimensions. Up and down, right and left for progress of melody, and thirdly depth, *i.e.* volume." "Then the pattern changed: the strands separated out, the lower patterns accentuated, the upper rippling." "Then came a pattern of a different type, beginning with zig-zags, obliquely transverse strands from lower left to upper right, going through a horizontally moving pattern." "I saw a frame," reports P., "containing a spiral growing larger and smaller. The spiral rotated: it had different coloured strands. The frame widened and narrowed, as the spiral changed in size." There is obviously a close analogy between the rise and fall of pitch, the blending, interweaving and segregation of different simultaneous themes, the motor effects of various rhythms and syncopations, on the one hand, and the forms and movements of geometrical designs, on the other[1].

---

[1] *Manual of Psychology*, 3rd ed. 685.

Such tendency to ascribe form and movement to a series of sounds is doubtless related to the not uncommon endowment of sounds with colour; one tone, one tone quality, one key, one word or letter or vowel appearing of one colour, another of another colour. The actual colour is in part, though probably not wholly, determined by early, long-forgotten, perhaps for some reason repressed, experiences. But the *tendency* to such coloured hearing runs in families: it is inherited, not acquired. Such colours appear in actual visual imagery occasionally in the replies of my subjects. Thus P. reports: "I saw a beautiful grey, a lovely grey with light shadows....I saw another grey, just lovely, like glacier water.... Don't you see the expanse, right on top of a hill?" H. reports: "At the pause I see a space, a grey misty space, clean-cut at each end, extending from left to right. This visual part is there all the time but it outlasts the auditory," *i.e.* when the music pauses the colour continues. Colours may be suggested not by the sounds alone but by the associations to which the music gives rise, and they may in turn suggest other associations. Thus, the just-mentioned 'grey' arose, explains P., because the music "reminds me of dawn—grey, fresh and nice." And having obtained the "lovely grey, I then saw a frieze, not a real good Greek one, but a Thorwaldsen or a Canova frieze."

"Following the pattern," says C., "is my greatest enjoyment in music. If I cannot follow it, I lose the beauty: I lose my bearings. There is no longer meaning in its movement. Right or left in the pattern may be compared to the behindness of the past. When the pattern is absent, the beauty lies in its clear-cut character of gloom, menace, and langour and simplicity." "I am now able to distinguish the parts of the individual movements. This enhances my enjoyment. I am able to follow it better. It makes the pattern clearer." He says that he tends to move with the pattern and is willing finally to surrender himself to it. Apparently his intra-subjective experience is helped by the pattern to the full appreciation and to the aesthetic enjoyment of the music. The music then appears to him in a form mid-way between a living person and a mechanical object. But when he can get no patterns of rhythmic or polyphonic foundation to enjoy, when, *e.g.* he listens to a simple melody or to a single tone, the character aspect comes to the fore with such descriptions as 'plaintive,' 'poignant,' 'child-like,' 'wailing.' "I might," he explains, "cry out at any moment, not from pain or sorrow. On the contrary I enjoy it." It would appear as if the pattern or other symbolic figure was a half-way elaboration of the object-matter, not reaching to the height and the free organic independence of the character aspect, but kept low by the trammels of associative and intra-subjective factors.

---

[1] *Op. cit.* 84, 85.

9. THE AESTHETIC VALUE OF THE PRAGMATIC AND OBJECTIVE ASPECTS.

The purely pragmatic and objective aspects in which the art material is considered in relation to its use and to the subject's standard of values are in themselves incapable of inducing the aesthetic experience. But they are indirectly of great importance. For the conception of a standard, although it cannot *induce* the experience of the beautiful, is of obvious use in forming a judgment of its aesthetic value. Thus we found that the subject F., despite the scant aesthetic enjoyment he derived from music, invariably showed himself correct in his judgment between good and bad music. To treat the art material as a mere inanimate object having a certain value in reference to the subject's standard is, as we have seen, merely a last resource in the case of the untrained; while in the case of the technician it is the consequence of his absorption in the material. It is the refuge of the untrained in the absence of the potentially aesthetic aspects of character, association, and intra-subjective experience. It is the resource of the artist, in his endeavour by repression to escape from the influence of the other aspects, in order, it may be, to attain the highest appreciable beauty of music—the beauty of musical meaning which is inexpressible in any other terms.

The pragmatic aspect may have considerable aesthetic value by entering into combination with the other aspects. The use of material may readily evoke the intra-subjective attitude; the appreciation of the *import* of the art object being followed by, say, impulses to do something with it, or by sensory or emotional changes in the subject, all of which may come to have aesthetic significance. Lastly, the personifying process, seen in its full development in the character aspect, may invest the changing melodic, rhythmic and harmonic forms, as we have shown, with a *quasi*-animate activity, *e.g.* of moving patterns or forms which may enhance the aesthetic enjoyment of the music.

10. THE AESTHETIC VALUE OF THE INTRA-SUBJECTIVE ASPECT.

Meaning of some sort there must be for us to have experience of beauty, and the lowest form of meaning which any stimulus takes is just the sensations which it directly evokes in us. Thus the lowest kind of beauty is experienced when the subject adopts purely the intra-subjective attitude, surrendering himself to the sensory, emotional and impulsive effects of the music. So long, however, as the subject gives himself up to the enjoyment of such experiences, all that he gets is pleasure or joy, not beauty. As Bullough rightly points out[1], a process of psychical 'distancing' is required in order that any of his sensations or emotions may appear beautiful. He must look on them with a certain detachment, to a certain extent impersonally. He has to project the beauty into his sensory, emotional or conative experience instead of subjectively appreciating the pleasure or joy to which they give rise. He has to look on them as a spectator, and in some measure at least to regard that experience as constituting in and for itself a living unitary, independent entity.

Next in order of development to enjoying his own feelings comes the listener's submission to following the enjoyment of the feelings of others, *e.g.* of the imagined performers or, in the case of the character aspect, of the music itself. With this must develop a sympathy, more or less perfect, with the experiences he is following. *E.g.* one of my subjects, E., reports: "I cannot feel emotion in listening to music, unless I feel that I am moving in the same emotional attitude as the persons (imaged)." And B. observes: "Then a long pause which seemed annoyed and confused. I participated in the annoyance and confusion. Then appeared a master hand to arise out of the forces of confusion. Then a revolt. Then once again he subdued them. Peace and triumph seemed to reign....But I had no feeling in myself of success."

---

[1] *This Journal*, 1912, v, 87–118.

How frequently the subject tends to identify his feelings with those of the creatures of his imagination, while listening to music, is well illustrated by the following quotations. J.: "I got the impression of people dancing, I think on the stage. I saw the moving figures, young people of both sexes. It struck me as a representation of a dance in the open air. There was a note of sadness among the dancers, a sort of regretfulness. I think that the sadness affected me and came secondarily to the stage sadness." Or again: "I saw one person alone to start with, asking for or expecting others to come and gradually a great crowd came running towards him. I distinctly felt I wanted to move with them."

Complete surrender may induce a state of transport or ecstasy. My Japanese subject, E., reports: "Sometimes I lose myself in the music.... I am unconscious and forgetful of myself." Another subject, K., reports: "I felt the effect of being carried away, partly emotional, partly strain and tenseness of body." But the surrender must be under voluntary control, or, as another of my subjects says: "I distrust them (the noisy parts) rather as an attempt to carry me away by mere force."

Thus complete surrender is incompatible with aesthetic enjoyment. The latter, as we have suggested, depends on a certain detachment of the art material from one's Self, so that the object is judged to be itself beautiful. That beauty may be attached to sensations, feelings, etc., is indicated by the following extracts from the notes of one of my subjects, E.: "The special feeling I get from music makes it beautiful. It gives me a tender poetic feeling, almost pity." Or as another, H., explains: "Certain short phrases gave me quite a beautiful thrill, localised in the diaphragm—like the feeling that early morning brightness gives one." When those feelings are regarded as living entities from the standpoint of an onlooker, they may be deemed beautiful as objects of experience.

11. THE AESTHETIC VALUE OF THE MEANING OF MUSIC.

So, too, the beauty of music may be derived from the story it suggests. "There she is," says P., "the little fairy. A sentimental sort of pantomime for children....Children dancing, not grown-ups....Men dressed in red with feather plumes. Don't you see the fairies? Yes. It's a sylvan sort of thing." No wonder that she asserts, "Music always gives me the sight of so many charming things. That's why I like listening to it." "I generally try," says E., similarly, "to make a story out of a piece."

The meaning of the music may be also expressed more generally, with reference to our affective 'attitudes.' Thus B. reports: "An insistent questioning and perhaps an unsatisfactory reply....A muffled knock which is disregarded. Then it took more definitely the form of someone's conscience being appealed to in vain....It came in words, 'You know you should.' Then with 'Can't you see I can't do it?' All quite impersonal. The man had yielded to the temptation, but had not succeeded in quelling his conscience. Still the voice of menace came pricking out during the enjoyment."

We have already alluded to the need for appreciating true sincerity and genuineness in music (p. 62). An illustration may be quoted from O. who remarks: "...in the middle its expression changed to being true to life, real stuff, though I did not fully understand it"; and from E.: "I felt that there was no reality in it. It was a mere mechanical imitation ...like a painting imitating a great master."

The importance of finding one or other of such meanings, before aesthetic experience is possible for some persons, is exemplified by such reports as B.: "No central idea in it. Never knew where I was," and E.: "Too much bothered about finding meaning to be able to see any beauty." In default of any other meaning, a subject is liable to revert to the more primitive forms of imagination. "The whole," says one subject, D., "has no meaning in the least to me. I don't understand it. I am catching hold of any image I can get." (Cf. p. 62.)

The more completely the meaning is concerned with the listener's notions of utility, the more impossible is it *per se* to get aesthetic appreciation. Whether we consider an art object well- or ill-fitted to express its purpose will not determine in us an experience of beauty. If we find it ill-fitted, this will debar us from readily experiencing beauty in it. If we find it well-fitted to express its purpose, our experience of beauty may be enhanced by our admiration and wonder. But the realization of appropriateness or perfection alone will not suffice to evoke an aesthetic experience. Nor will the mere following of analysis and appreciation of musical form. The thing of beauty must be regarded not as a satisfying piece of man-made mechanism, but as a living organic whole, without direct reference to our own value and use of it.

But there is always a meaning in music apart from what may be obtained from feelings, stories, actions, colours, patterns, and language. Recognition of this fact becomes clearer with the development of musical appreciation. "When I see the pictures [the images evoked] they take up almost all my attention," protests L., "so that I have the feeling 'Dear me! I'm not listening,' and then I get back to the music." So, too, one of my subjects, C., objects: "I cannot...conceive music *saying* anything;" and another, H., explains: "Music has a meaning, but always in musical tones. I couldn't put it into words. It always irritates me to be asked to do this."

It need hardly be pointed out that when there is a possibility of so many different sources of meaning, some present in consciousness, others inhibited, but each tending to some particular response on the part of the listener, there must be a general harmony between these various meanings for aesthetic enjoyment to reach its climax. Likewise there must arise the ability to grasp the musical piece as a whole. Whereas painting is presented in space, music is presented in time. Whereas we seldom find that *parts* of a painting are beautiful, in music we are apt to get aesthetic experience here and there; and it is only after the highest flight of synthesis that we can find enjoyment in the beauty of the music as a whole. "I felt," says H., "the whole piece as one. I felt the player conceived it as a whole." "This part," says another, K., "jarred and worried me, because it didn't seem to fit in with the other." A third subject complains: "I haven't grasped it as a whole." "Not an artistic whole," is the criticism of a fourth subject, J. "I thought," says yet another, K., "of the general shape and balance of the whole....I liked the contrast of the two sections." "Making notes [of introspection]," objects D., "made me only see the pieces in bits."

12. THE IMPORTANCE OF 'DISTANCE.'

We can see now how the various aspects which we have distinguished in the listener may each play a part in the awareness of beauty, and how the different fundamental connexions of music, with courtship, with dancing, and with rudimentary language, may each contribute to aesthetic enjoyment. These different connexions may be differently stressed in different persons to-day so that one tends specially to sexual, another to dramatic, another to verbal associations with music. But we come to recognize that apart from these connexions music may be appreciated for its own inherent beauty, that is to say, apart from its sensuous, emotional or conative influences and from associations, symbols and the products of 'animistic' characterizations. The one common and essential attitude required for aesthetic enjoyment is one of detachment. The listener must view the music, as Bullough rightly insists, from a certain psychical 'distance.' If that distance be excessive, as occurs in listening for the first time to exotic music or to other unfamiliar styles of music, the subject feels too remote to get, as it were, to grips with the art material. It is over-distanced. On the other hand, it is under-distanced, when he surrenders himself wholly to its influence in such a way that he is a more or less passive instrument, played upon by the music, interested in his sensations, images, emotions or impulses, solely in so far as they have immediately personal and 'practical' import.

13. THE IMPORTANCE OF THE 'MYSTIC' FEELING.

There are three main lines of activity which take us away from the purely practical, every-day aspect of our experiences. The simplest and most primitive is play. This fundamentally consists in giving a fictitious value to our motor behaviour. The second is phantasy, in which, as in day-dreaming, wrapped up in our selves, we allow our imagination full liberty, regardless of the realities of our environment. The third consists in mystical experience in which we lose the normal awareness of our own individuality and of its relation to our surroundings. The ecstasies (active and passive) of love and religion afford the most striking and undoubted instances of this kind of experience—the lost relation of the self to its environment. I believe—and this belief is supported by the introspective evidence of my subjects—that our experience of beauty always partakes in some degree of this mystical or ecstatic character. Nowhere in art or nature as in music do we more keenly feel this 'uplifting of the soul' as we term it, or as we may come to term it, this 'uplifting of the unconscious.' But the mystical or ecstatic feeling must not be allowed to go too far; otherwise we are carried away beyond our ability to experience beauty. On the other hand, unless we do, in however small degree, surrender our practical every-day attitude that defines the relation of ourselves to our environment, unless we feel ourselves in that mysterious poetical atmosphere, I do not believe that beauty can be experienced, whether in music, painting, sculpture, architecture or dancing, whether in imagination, in a mathematical problem, or in a purely sensory or emotional experience.

(*Manuscript received* 12 *May* 1921.)

AGE STANDARDS FOR THE SEPARATE NORTHUMBERLAND TESTS.

BY GODFREY H. THOMSON.

(From the Department of Education, Armstrong College, Newcastle-upon-Tyne.)

IN the last number of this *Journal* there was described an hour's intelligence test given to about 3000 children in Northumberland, and standards of performance in the whole test were there obtained.

The booklet of tests consisted of six different kinds of test, given in two cycles, A 1 to A 6 and B 1 to B 6. Owing to printing requirements the order was not quite the same in the two cycles. The following table shows the tests and the point scores possible on each:

TABLE I.

Name of test	Number in booklet	Possible score
Extra Word	A 1 + B 1	8
Number Series	A 2 + B 2	10
Schema or Family Tree	A 3 + B 4	11
Middle Word	A 4 + B 5	8
Extra Number	A 5 + B 3	9
Hindustani	A 6 + B 6	14
		Total 60

The age standards given in the previous article for the whole test were based mainly on the marks obtained by those children whose position in school was that of normal children of their age, though the scores of the other children (in a class in advance of or behind their proper school class) were also considered. In the present analysis it has been thought sufficient to retain only the children of the former category of whom there are here considered the following number of cases:

TABLE II.

Age	School Standard	Number of cases
10 but not 11	IV	111
11 „ 12	V	506
12 „ 13	VI	395
13 „ 14	VII	50
		Total 1062

These numbers of cases differ slightly from the numbers running diagonally across Table II on page 212 of the previous article, but only because of accidental reasons, a few extra cases having on the one hand come in since then, and on the other hand a random set of papers having been left out of this present count owing to the work of the team of some 40 students having to cease at a certain hour.

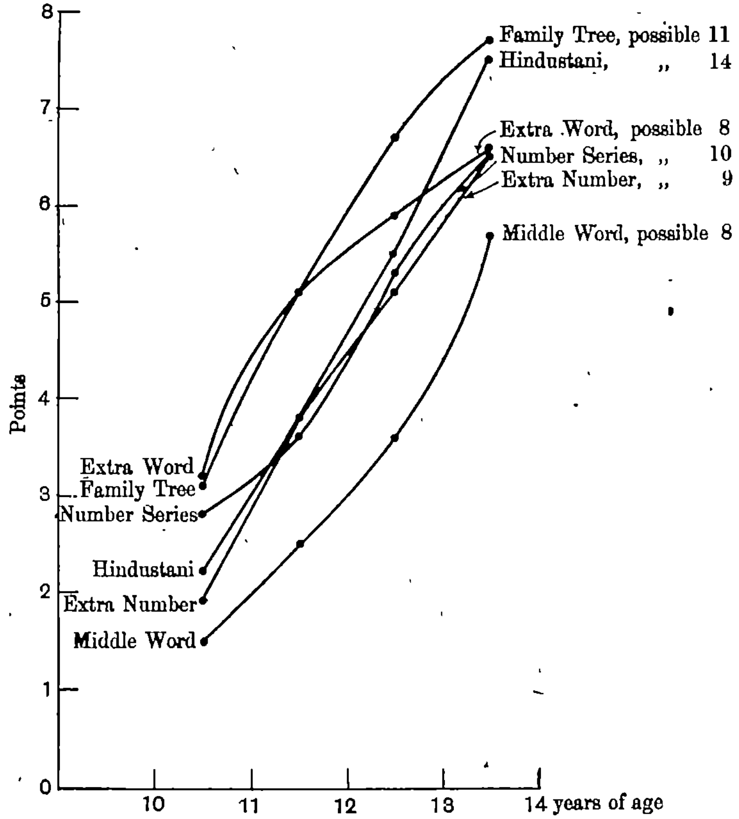


Fig. 1. Actual Average Scores in each Test.

The hour's test was given by simply placing the booklet in front of each child and allowing him (or her) to tackle the problems at his own speed. There was no time limit set to each test but only to the whole, though the cyclic arrangement was intended to assist in a reasonably equal division of time.

The age standards of performance in each test, as given in Table III and Figs. 1 and 2, are therefore only to be understood as applying to such conditions. The average scores of each age group, or standard scores, are shown in the left-hand part of Table III and again as percentages in the right-hand part of that Table.

TABLE III.

Average scores in each test at each age.

	Actual scores					As percentage of possible			
Age in years	10½	11½	12½	13½	Possible	10½	11½	12½	13½
Extra Word	3·2	5·1	5·9	6·6	8	40	64	74	83
Number Series	2·8	3·6	5·3	6·5	10	28	36	53	65
Family Tree	3·1	5·1	6·7	7·7	11	28	46	61	70
Middle Word	1·5	2·5	3·6	5·7	8	19	31	45	71
Extra Number	1·9	3·8	5·1	6·5	9	21	42	57	72
Hindustani	2·2	3·8	5·5	7·5	14	16	27	39	54

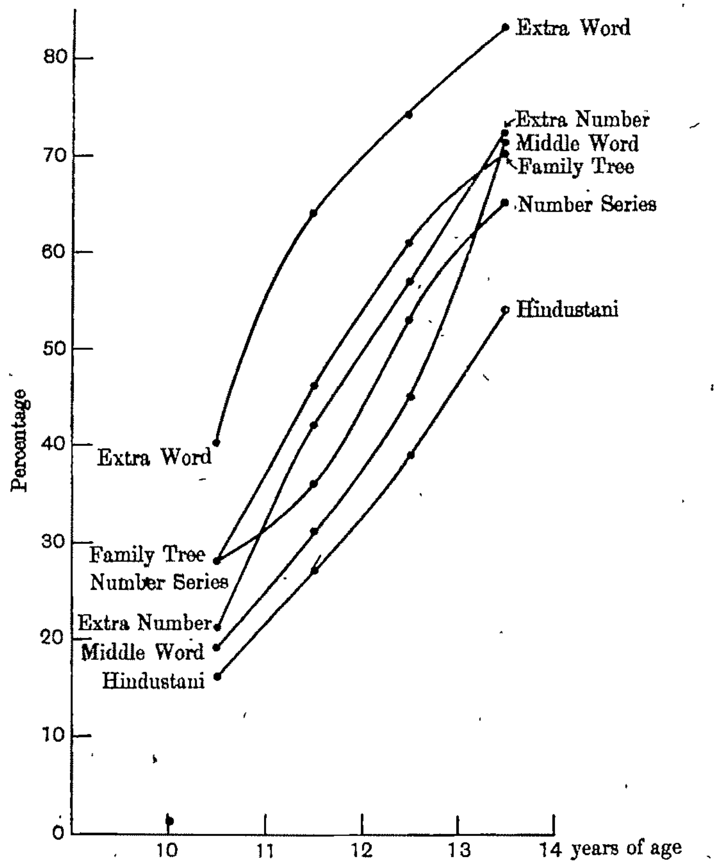


Fig. 2. Scores as Percentages of the Possible Scores in each Test.

The accompanying figures show these results graphically. From Fig. 1, which shows the actual scores, can be seen readily in what manner the total score at each age was made up, 'Family Tree' giving the largest score at all ages except the youngest. 'Hindustani' gives a small proportion at 10 and a large proportion at 13, and is the steepest curve. Fig. 2 shows the results as percentages of the possible score for each test. 'Hindustani' is throughout the hardest, and 'Extra Word' the easiest. The latter and 'Family Tree' are convex upward, showing that they are approaching their limit. The concavity of the 'Middle Word' curve is probably due to the setting in at the top age of the solution of the double series of words given in B 5 (see booklet, in former article, page 207). Theoretically, the curves ought to be ogives, concave upwards below 50 per cent. and convex above. 'Number Series' shows both these points but I think it is accidental at the lower end. 'Hindustani' is below 50 per cent. and is slightly concave, as it should be.

I wish to thank my staff and a number of my students for entering almost 32,000 scores in the separate tests on forms in such a manner as to facilitate my final analysis of the statistics.

(*Manuscript received* 15 *December* 1921.)

THE BIOLOGICAL AND SOCIAL SIGNIFICANCE OF THE EXPRESSION OF THE EMOTIONS.

By CAMILLE NONY.

(*From the Psychological Department of the Sorbonne, University of Paris.*)

A. The Biological Significance.

I. *The term 'expression' of the emotion* (pp. 76, 77).
II. *Dependence of the emotional reaction on* (i) *the intensity of the emotional shock*, (ii) *the experient*, (iii) *the emotion* (pp. 77–80).
III. *Theories of the biological significance of emotional reactions. Dumas's denial of their purposefulness* (pp. 80–83).
IV. *The utility of emotional reactions* (pp. 83–86).

B. Development of the Social Significance.

I. *The stage of emotional reflexes* (p. 86).
II. *The stage of acquisition of meaning of the reflexes* (pp. 87, 88).
III. *The stage of voluntary use of the reflexes to convey meaning. Dependence on extent of* (i) *powers of voluntary innervations*, (ii) *experience, and* (iii) *indirect appeal* (pp. 88–90).
IV. *Social formation of schemata of expression* (pp. 90, 91).
V. *Summary* (p. 91).

A. The Biological Significance.

I.

When an emotion is experienced, such organic phenomena as weeping, trembling, pallor, blushing, etc., can be observed at the same time. These phenomena are called the *expression* of the emotion. This term logically implies that emotion is a mental fact previous to and provocative of the organic phenomena; but if we accept the James-Lange theory we assume on the contrary that the physiological reactions are the cause of the mental experience, the primitive and essential facts without which there would be no emotion. The term 'expression of the emotions' applied to these reactions has therefore no longer any meaning, since the so-called 'expression' is in reality the emotion itself. But as we are not here concerned with the problem of the *nature* of emotion, we use the term 'expression of the emotions' without assuming any particular theory which it may imply. We use it simply in the general sense which we think may be acceptable both to the dualistic and monistic points of view, namely that the expression of an emotion is the sum of the various bodily reactions that accompany it.

II.

Experience shows that emotional reactions may vary in number and intensity, and may be dynamogenic or inhibitory—(i) according to the *intensity* of the emotional shock; (ii) according to the *individual* who experiences it.

E.g. a given individual *A* is afraid; if his emotion reaches a certain intensity, there will be active reactions, a general bodily excitation, which will consist of:

Respiratory changes		(acceleration of its rhythm).
Vascular	,,	central: *e.g.* cardiac acceleration peripheral: *e.g* vaso-dilatation.
Trophic	,,	(altered metabolism[1]).
Secretory	,,	(the phenomena may be general or restricted to a few glands).
Motor	,,	movements of the body: these may vary from slight trembling to precipitated flight particular facial movements horripilation.

For this individual *A*, and at this given degree of the emotion, fear will be dynamogenic.

For the same individual *A*, fear to a higher degree may provoke paralysis and abatement of all physiological functions.

For an individual *B*, fear even in a feeble degree may always be inhibitory.

The same analysis could be made for other emotions, showing in the same way that anger, joy, sorrow, etc., may, according to the individual and to the intensity of the shock, be active or passive.

(iii) according to the *emotion*. Obviously the total reaction is not the same in the case of all emotions, and the question arises,—are any of the reactions common to all or specific to any particular emotion, *i.e.* is the total reaction partly common and partly specific or is it entirely specific? If we consider only the functional excitations or inhibitions, without regard to the qualitative or quantitative differences which may exist between the reactions according to the emotion that has evoked them, these reactions may be classified thus:

---

[1] The effects of these changes in the general nutrition are still more obvious in the pathological emotional states, such as melancholia.

(1) The strictly personal reactions, such as are called forth in some individuals by all emotions.

[It is a fact that each individual has his own way of reacting to an emotional stimulus, this reaction being always the same whatever the emotion may be and revealing some weak point of his constitutional make-up. This naturally varies with the individual: in some the disturbances called forth are especially those of the vaso-motor system, in others of the respiratory, with still others of the digestive system, and so on.]

(2) The reactions common to all individuals—(*a*) and to all emotions:

In fear, which is a very intense and general emotion, all functions are affected (whether of excitation or inhibition); so also in intense anger. And from the above standpoint we may say that all reactions are common to both these strong emotions. But there are also more restricted emotions (the emotion provoked by the sex instinct, the emotion experienced by a hungry animal at the sudden sight of food—which we shall henceforth call the 'feeding' emotion) which concern but one function apiece, and in which, however, as a result of the diffusion of the nervous discharge, other functions than those immediately concerned also become affected. Indeed, any emotional shock whatever, feeble though it be, is revealed in the modification of the normal respiratory, sphygmographic and plethysmographic curves, and if the emotion attains sufficient intensity it will provoke such secretions as sweat or tears, or call forth unusual movements, or on the contrary it will entirely suppress every movement, thus producing abnormal immobility.

(*b*) And particularly to special emotions:

There are reactions which can be provoked only by a specific stimulus: in the sexual emotion or the feeding emotion, the specific secretory responses correspond to the specific stimulus. The local secretions are occasionally accompanied with generalised secretions (polyglandular expression), these being not specific but resulting from the diffusion of the excitation.

[It should be noted that these local secretions, though usually specific, may also in certain individuals be not specific if they happen to coincide with the personal common reaction.]

This classification is based, as we have said, only upon the functional modifications accompanying the emotion and has no regard to the quantitative or qualitative variations that may occur in these changes. But such a standpoint is too general and would lead us to absurd conclusions. Indeed, if, for instance in the movements accompanying some active emotions, the excitation alone is considered, they will all be confused under the common name of motor excitation, though these are very different according to the emotion. Besides the excitation, the mode of excitation must be considered. The reactive movements will be very different according as they form part of fear, anger, joy, or sorrow, etc.

They have a different form and direction; they constitute the framework for very different kinds of behaviour; they are normally connected with different feelings.

Thus anger implies an offensive attitude, movements of attack more or less marked; fear, a defensive attitude, movements of flight and retreat from danger; joy, numerous, quick and friendly movements; grief, movements of despair, the hair being torn out, the hands twisted, the clothes rent.

This differentiation is still more obvious if we consider the muscles of the face, which are smaller and come more often into play, are more independent of each other's action, and consequently give rise to movements much more expressive than other muscles of the body. The muscular movements of the face (as of the rest of the body) therefore give rise to a special picture or schema for a given dynamogenic emotion, and this characteristic picture of the emotion is manifested by all (normal) individuals.

We arrive at a similar conclusion upon considering the emotional picture which is furnished to us when movements are inhibited: the attitude assumed will differ according to the emotion experienced, as will be seen in comparing, say, the picture presented by the momentary paralysis in cases of extreme fear and surprise,—this difference being more obvious in the muscles of the face than in those of the trunk and limbs.

We may wonder whether the above distinction between the mere muscular response and the way in which it takes place in space and time cannot be applied to the other reactions of the same group [No. 2 (*a*)] and whether, though commonly occurring in emotions of all individuals like the muscular reactions, they do not also occur in a different way for each emotion.

In both active fear and active anger, we find respiratory and cardiac acceleration, vaso-dilatation, etc. But will these changes be identical in both emotions? Though in both cases they will show excitation, will they not be influenced by the general motor attitude, which is defensive in the first case, offensive in the second? Is not the rhythm of the respiratory movements, the rhythm of the heart beat or pulse, more or less quick and irregular according to the emotion?

Are the non-specific secretory responses always qualitatively and quantitatively the same, whatever the emotion may be? Do tears, say, flow identically in joy and sorrow? Is not the rate with which they flow, as well as the concentration of the secretion, influenced by the general organic state and the psychic content, which are here so opposite? It seems likely that, just as Pavlov noted variations in the amount and chemical composition of the gastric juice secreted by his dogs when psychically stimulated, we could, if we had the means, establish that a secretion has a different chemical composition, and is more or less abundant, swift, regular or spasmodic, according to the emotion.

We have no positive experimental data to present in order to confirm this view, but it is surely probable that a reaction commonly called forth in several emotions has, in a given emotion, something specific in its *modus operandi* that distinguishes it from the same reaction occurring in another emotion. It would be an interesting experiment to make. But if our hypothesis is right, organic changes, though varying more or less with the individuals, will always show specific modifications characteristic of the emotion.

The expression of an emotion is therefore doubly specific: it is specific to the emotion[1], and specific to the individual by virtue of the personal reaction.

III.

In seeking the biological significance of the emotional reactions let us first remark that the authors who have tried to solve this problem have not always spoken of the same thing. Some have only considered the rôle of 'mimetic' expression as if other reactions had no rôle at all; and besides, those who have concerned themselves with the mimetic responses have frequently attached different meanings to the term: some include muscular movements of the whole body (this will be *our* meaning), some of the face alone, still others would include also weeping.

Now, some authors, basing their opinions upon Bechterew's experiments[2], seem to separate the mimetic responses from the expression of the emotions. It is certain that in animals deprived of their cerebral cortex the mimetic reactions are still produced, and in a very complete and adapted fashion, under the influence of an external and immediate excitation, for the mimetic centres (of the optic thalamus) are intact. There is here expression without emotion. Inversely, in patients whose cortex is intact and thalamus injured, there is emotion without mimicry. But if both mechanisms are physiologically independent, it is nevertheless true that in normal individuals emotion and mimicry are so connected and that the one involves the other.

Let us next consider some of the more important theories that have been hitherto advanced as to the biological significance of the expression of the emotions.

---

[1] This statement does not mean that by observing the specific modifications of the respiratory or circulatory tracings we could deduce with certainty the correlative psychic phenomena: the variations which occur in the reactions according as these accompany one or another emotion, are insignificant compared with the difference which exists between the two psychic contents. Whereas the key-board of the organic reactions has but a relatively small number of notes and but limited possibilities of combination, the psychic key-board is immeasurably rich and allows of an infinite number of shades and combinations.

[2] *Les voies de conduction du cerveau et de la moelle*, 1900.

Darwin believes in the 'purposefulness' of the emotional reactions in so far as they come under his Principle I (persistence of useful habits acquired in the course of evolution): (i) immediate purpose, (ii) persistence of a past purpose, or (iii) purpose by association.

E.g. (i) The self-display and bright colours of the male during the mating season have two objects,—to frighten other males by showing his strength, and in that way to keep them off; to allure and to charm the females by his beautiful appearance, thus insuring an easier victory.

In anger animals assume an aggressive attitude: feathers or hair are spread wide, the weapons of offence, teeth, claws, etc., make ready for fight; and all such movements of attack, which may vary from a mere gesture or attempt to the complete execution of the act, have for object the terrifying of the opponent at the sight of a formidable enemy.

(ii) In man, anger gives also rise to the same movements of offence, though they are now only indicated: the fists are clenched, the hair stands on end, the teeth are shown, the nostrils dilate. All these movements were useful to our remote ancestors in the struggle for life, and what we see now in the expression of our contemporaries when angry is a weakened persistence of formerly useful habits.

(iii) When we desire to observe an object carefully, we frown because in so doing we concentrate our attention by restricting our field of vision. By analogy we also frown when, in order to see more clearly intellectually, we concentrate our attention and thereby restrict our field of consciousness.

For Darwin, the purpose of the emotional reactions is a conscious purpose, *e.g.* the anger reactions are attitudes consciously assumed for the purpose of giving the individual a terrible appearance before the enemy. But, contrarily to Bell and Duchesne, he does not admit the existence of specialised muscles of expression, arguing with humour that the muscles of the face being nearly identical in man and in monkey, it is quite incredible that these were specially created to enable the apes to make their hideous grimaces.

Spencer believed, like Darwin, in the purposefulness of most of the emotional reactions. But, though an evolutionist too, his interpretations of the facts often differ from those of Darwin.

Wundt and Mantegazza, whose principles of the expression of the emotions resemble very closely those of Darwin and Spencer, naturally also assumed the principle of purpose in the emotive reactions. But their interpretations, furthermore, are often very fanciful and far-fetched, and consequently much more improbable.

Bechterew, though he agrees with Darwin as regards the question of purpose, nevertheless does not believe that the purpose is conscious;

e.g. the anger reactions have not the conscious purpose of inspiring fear to the enemy. If it were so, it would mean that animals have the faculty of self-criticism and deduction. So, too, when the males clothe themselves in bright colours in spring, it is not for the purpose of insuring an easier victory, for that would suppose that animals have the faculty of aesthetic judgment and calculation. In neither instance are we justified in supposing that animals are capable of such rational processes, or at least that—if such processes exist at all in them—they are sufficiently developed to be conscious operations. In fact, each one of these reactions is the necessary consequence of a specific excitation. "Tous les mouvements mimiques ont un sens biologique, non d'utilité, mais de nécessité, comme réactions aux impulsions reçues....Qu'il s'agisse de mimique sexuelle, de gestes de défense ou d'agression, la nécessité biologique règle tous les détails, sans concours d'aucun facteur personnel ou volontaire[1]."

Indeed, Bechterew's experiments demonstrate that mimetic reactions are automatic, as we have seen previously, but it cannot be concluded that the denial of a conscious finality implies that of an unconscious one: for the reflex mechanism may be adapted in view of the conservation of the individual or of the species.

G. Dumas[2] rejects any idea of finality, even unconscious, and presents a purely mechanistic explanation of the expression of the emotions. The reactions which Darwin, Spencer, and above all Wundt and Mantegazza have often explained so unnaturally, result merely and necessarily from a local or generalised excitation or inhibition. Thus the smile results simply from the upward contraction of the corners of the mouth under the influence of a slight excitation of the facial nerve, whether this excitation be of internal and psychic origin, or of external and experimental origin (application of a feeble electric current), or of pathological origin (facial palsy, hysterical contraction). Finality is a construction of our imagination taken over arbitrarily from biological laws of which we desire to find at all costs a practical explanation. For Dumas there are no muscles of expression but only muscles each having a determined rôle in the life of the organism; *e.g.* the muscles which are said to have been specially created in order to produce the smiling expression are in reality masticatory muscles. Indeed, the experiments of Mosso long ago demonstrated that an emotional reaction is only the function of a given excitation. The application of a feeble current to the facial nerve of a dead dog produced first the contraction of the muscles of the forehead and ears (mimicry of attention); and as the excitation increased in intensity the following reactions were added,—contraction of the muscles of the nose, eyelids and cheeks, then contraction of the upper lip, the teeth being thus shown (mimicry of anger); a still stronger excitation made the jaws open as if to bite[1].

---

[1] Bechterew, *J. de Psychol.* 1910, 385.

[2] *Le sourire*, 1906. Cf. also "L'expression des Émotions," *Rev. philos.* Jan. 1922, p. 45.

IV.

I accept in its major outlines this last positivist conclusion and I shall state as my own point of view that the emotional reactions are the mechanical result of a nervous excitation or inhibition. The biological laws, though they are obviously favourable to life (otherwise they would not be *biological* laws and there would be no life at all), nevertheless have nothing in them which permits us to conclude the existence of a Providential finality through which every detail of behaviour, even the apparently most insignificant, would indicate a purpose predeterminate in the 'scheme of creation.'

Let us take, for instance, the vaso-constriction that follows the first instant of an emotional shock. Some authors regard this as a case of direct finality: the reaction has as its object the reduction of the peripheral circulation for the benefit of the cerebral circulation, so that the brain, being better nourished, may be fitter for the work of adaptation which the new event demands of it. But this explanation is contradicted by experience: François Franck has indeed demonstrated that cerebral activity and an increase of cerebral circulation do not condition each other[2].

Let us next take the instance of weeping. For some authors, it is a case of finality by association. We weep in moral pain, according to Darwin, because the secretion of tears results from the contraction of the periocular muscles, which contraction takes place in physical pain in order to prevent the eye from being engorged with blood (principle of useful habits); finally, any kind of pain will be capable of provoking the secretion of tears, so that weeping—according to Darwin—is a conditional reflex. According to Wundt our psychic tears are produced by analogy with our physiological tears: the physiological function, he says, of tears is to sweep away the dust from the surface of the eye, and in the same way our psychic tears sweep away the painful representations (principle of the association of movements with their sensory representations).

---

[1] Mosso, *Fear*.

[2] Operating upon a subject with an opening in his forehead and having previously given him an oxygen-ball to inhale in order to allow him to stop breathing for a certain time, he then ordered him to do some intellectual work, such as reckonings, and no modification in the cerebral pulse was to be seen. Fr. Franck arrived at the conclusion that the changes noticed by Lange in the cerebral pulse during intellectual work were due to the well-known influence of the respiration upon the circulation. (Quoted by G. Dumas in *Rev. philos.* March 1922, p. 240. For further information see Fr. Franck, Cours du Collège de France, 1904, also a communication by the same writer to the International Congress of Medicine, 1900, Physiological Section.)

Such sophisticated and fanciful explanations vanish before the fact that tears are a secretory reflex which is called forth (*a*) by external and physical excitation of the eye or of its surrounding parts, (*b*) by internal excitation of the centre of weeping (in the thalamus), this excitation having either a pathological origin, or resulting from the motor excitation of the cortex (psychic tears).

And what is to be thought of Mantegazza's explanation of emotional trembling, when he says that such trembling is "obviously" very useful, for it heats the blood which otherwise might become cold under the influence of the emotion?

What purpose can we find in the general organic excitation, say, in active joy? The movements are quick and numerous—children leap and clap their hands—the hands and feet are warm, the cheeks are round and full, the eyes are bright and move quickly, the corners of the lips are slightly uplifted as in a continual smile, language flows in a smooth and easy voice. All these active reactions simply result from the muscular hypertonia, better circulation and increased anabolism, and from numerous and easy associations of ideas. They are the signs of an increased physiological and psychological activity, but such increase of activity is not obviously in any way useful.

The question is not whether the emotional reactions are useful or not: they just exist and cannot but exist. Some, as tears or blushing, do not cause much trouble to further behaviour, others, like trembling, may be frankly said to be harmful in most cases. As a rule the emotional reactions are never useful, but they may be indifferent or more or less harmful.

This being said, that it is idle and unscientific to search with a bias for finality in the emotional reactions, and having demonstrated that a true knowledge of the facts destroys the delusion of finality in these reactions, we shall say now that this is a general statement to which some privileged reactions seem to make an exception. This is the case, we think, in the four emotions directly connected with the instinct of conservation under its defensive form (fear), offensive form (anger), affectional form (sexual emotion) and vegetative form (feeding emotion).

The backward movements and defensive gestures in fear, and the forward movements and aggressive attitude in anger, seem to be reflex mechanisms adapted to a purposeful aim; though here one may object that, being naturally inclined to search for finality everywhere, and in active fear, say, on the one hand noticing the movements of flight of the individual who experiences the emotion, and on the other hand seeing the immediate danger, the cause of the fear, or deducing its existence from the movements of retreat,—between the two facts we throw the bridge of a finalistic explanation: these movements are executed *in order to* escape from danger.

As our judgments are necessarily limited by the constitution of our minds we cannot say, owing to the lack of a standard which could serve as a basis for comparison, whether in this case finality is objective or only subjective; but in any case it seems that these movements *have* a finality by themselves, independently of our interpretation. In active fear and active anger the movements of retreat and of attack are often really efficient. But let us notice that this usefulness can however have but a relatively vague character, for we deal here with reflex actions, *i.e.* actions adapted only to a very general aim (*e.g.* to escape from danger in case of fear) and not to the particular circumstances of a determinate case as would be a rational action. But it often happens that the reflex response and the consciously and voluntarily orientated action combine: the reaction occurs originally as a purely reflex response, *e.g.* backward movements in case of fear, and then may be consciously changed to a purposeful flight, the reflex response being in this case directly useful as preparatory to the reasoned action.

In passive fear has the motor inhibition, and in passive anger has the same motor inhibition, *plus* weeping, a finality? Darwin answers that such immobility has the object of counterfeiting death and thus keeping off the enemy by inspiring disgust. This seems to me an unnatural explanation which is anyhow available in only a very few cases. Etienne Rabaud has demonstrated that it is a case of inhibitory reflex[1].

The reactions of passive anger have no finality. If tears may be termed useful because they occur from efferent excitation which would provoke nervous disturbances should it find no way out, the real aim of anger is missed since no movement happens. Moreover the secretion of tears does not occur for the purpose of helping out in a harmless way the motor excitation: it simply results from the connexion between the centres of movements and the centres of tears, and if the anatomical connexions between the various parts of the brain were different, a motor excitation of the cortex might lead to a secretion of saliva, say, or any other organic phenomenon. What then would be the use of this reaction?

It is in the two other emotions (the sexual emotion and the feeding emotion) that the reactions are most obviously finalistic. These consist of (i) internal and localised secretions, (ii) the specific behaviour that these secretions determine through the medium of the correlative coenaesthesis. These reactions are unquestionably useful as preparatory to the actions which are about to follow. In the feeding emotion, in particular, the finality of reactions (i) and (ii) is brought to light by the experiments of Pavlov: not only may the sight of food determine in a hungry dog an abundant secretion of gastric juice (psychic digestion), but the chemical composition of the juice is such that it is precisely the best one to insure the best digestion of the food.

The useful reactions in the above four active emotions are those we call the characteristic ones. But besides these characteristic reactions have the others some finality?

E.g. In active fear or anger, what is the use of the respiratory, vascular and secretory changes, and of the diffuse motor excitations—from which we except the specific backward or forward movements? In the feeding emotion what is the use of the respiratory and vascular changes, of the secretions other than those of the digestive system, of the movements other than those of prehension and mastication?

All these reactions result from the emotional diffusion of the nervous excitation, and as such cannot be useful. If the characteristic reactions have a finality, it is because they constitute the specific response to a specific stimulus. Far from being useful, the other reactions are harmful because they interfere with the useful effects of the specific response; and when they happen to predominate, the emotion is at its strongest; but there is no longer finality, all the physiological functions of the organism are upset, and serious disturbances may ensue. On the contrary when the finality is at its clearest it is when the specific response only takes place, but then there is no longer emotion. Indeed, in a reaction strictly adequate to the stimulus, there is no room for emotion; it is just because there is diffusion of the excitation that emotion developes. The efficiency of the specific response varies inversely with the diffusion, *i.e.* with the emotion.

---

[1] Et. Rabaud, "L'immobilisation reflexe et l'instinct de la simulation de la mort." *J. de Psychol.* 1920, 865.

In general, to search for finality in the emotional reactions is to misconceive the essential characters of emotion, which consist in disorder and mal-adaptation. If fear, anger, the sexual emotion and the feeding emotion have some finality, this is only when they are of moderate strength. Moreover, in so far as their reactions are useful, they are not emotions but physiological functions.

B. DEVELOPMENT OF THE SOCIAL SIGNIFICANCE.

From what precedes one can already foresee that the mechanistic point of view is insufficient to account for the expression of the emotions. We have here to take into consideration that, by definition, an expression has a social aspect, and therefore may have a social action and in turn be modified by social influences. As a matter of fact the two aspects of the expression of the emotions, the biological and the social, are now so closely associated in our minds and in our experience that writers have often confounded them in their explanations and have not seen that they are in their origin distinct phenomena, the second growing out of the first but being in no way predetermined by it. This evolution from the biological to the social results from the social circumstances surrounding the expression of the emotions: we live in the companionship of men and animals, we observe in others the expression of their emotions and feelings, and they in turn in us.

Three phases can be distinguished in this evolution:

I.

At the basis there is an involuntary biological mechanism: emotional reflexes, specific
- for the emotion,
- for the individual (personal reaction).

II.

Anyone observing the specific expressive complex is able to interpret the correlative psychic state of the subject by a double process of induction and deduction, the one by supposing that others have the same reactions as himself for a given emotion, the other by projecting his personal experience upon others and then concluding, from the external manifestations of the emotion, the emotion itself. This method is instinctively used by everybody. In one of E. Poe's Tales[1] a schoolboy says: "When I wish to find out how wise, or how stupid, or how good, or how wicked is anyone, or what are his thoughts at the moment, I fashion the expression of my face as accurately as possible in accordance with the expression of his, and then wait to see what thoughts or sentiments arise in my mind or heart, as if to match or correspond with the expression."

But this process has its limits for it is based upon the identity of the biological organization in all individuals, and all individuals do not react identically to the same emotion: the personal reaction which, in a given individual, always affects the same physiological function whatever the emotion may be, therefore varies with the individuals; in the pathology of the expression of the emotions (paramimicry, troubles caused by lack of inhibition or lack of dynamogenism) the expression does not correspond to the same inward emotion as in normal individuals (*e.g.* the patient will weep or make gestures of anger when in reality he is joyful), so that the usual deduction leads to a wrong interpretation, the same reaction in the subject not corresponding to the same emotion in the observer. An education is needed; when we have learned, by a series of experiments, that in a given person a given manifestation corresponds to a given psychic state, then we shall be again able to use with success the deductive process. In the same way, when we deal with the expression of an emotion which we have never experienced, a certain education is needed in order that we may grasp the link between the outward manifestation and the inner state of which it is the sign. This education usually takes place as soon as the child begins to observe, so that the deduction becomes very soon unconscious. All our interpretations of the behaviour of men and animals—as well as of the facts of the biological and physical world—are in the same way apparently intuitive and constitute the stock of our practical knowledge. But these 'intuitive' judgments which determine our social behaviour are nothing but the last term of an unconscious deduction: the conclusion of the syllogism comes alone to the light of consciousness, the two first terms remaining in the dark.

---

[1] "The Purloined Letter." Edinburgh: A. & C. Black, 1874.

This education once accomplished, *i.e.* when the child has built up his theory of signs, the deductive process can be relied upon—on the condition that we have neither made any errors in our observation nor in our reasoning, and in so far as the reactions are merely physiological, *i.e.* uncomplicated by any social influences, and to the extent that we limit ourselves to dealing with normal people. This method can be applied to all men and even to all animals, for all living beings are to some extent biologically similar. The reflex manifestations of fear, anger, etc., are alike in all men and animals of all species, and it is this physiological analogy of animals of all kinds and men which is implicitly admitted even by people most ignorant of physiology when, before a cat with bristled hair and arched back, a bird with raised crest and feathers stretched wide, a man with hair erect, they conclude that the cat, the bird and the man are angry. It is because animals have, in emotion, a behaviour analogous to our own that we can understand the psychological meaning of their emotional reactions, their affective psychology, and this is due to the resemblance of their physiological organization to our own. Let us remark that our animal psychology is often interspersed with anthropocentrism, but here, as regards emotion, the anthropocentrism is at least based upon a real community of nature.

III.

He who undergoes an emotion is conscious not only of the reactions which he experiences but also of the effect which these produce upon those who observe him, and specially of the effect produced by the motor responses which are the most visible and the most interesting socially for they can modify the outer world: they act upon it directly (*e.g.* a blow occasioned by a fit of anger), or indirectly as a warning indication of an action that may follow (*e.g.* the clenching of the fists), and provoke in the observer the proper reactions: in this instance gestures of defence or retreat.

He who has been conscious of the effect once produced by his emotion, through the medium of expression, will seek on another occasion—if such be his interest—to make believe that he *is* experiencing this emotion, and for this purpose he will use the social sign of this emotion: its expression. He will voluntarily use the mechanism spontaneously set in motion in an actually and previously experienced emotion; he will imitate the action he executed while under the real emotion.

[*E.g.* if he has any interest to look sad he will assume a drooping attitude—drooping head and hands, bended knees,—he will pull down the corners of his mouth and thus his cheeks will be flatter, his face longer, his eyes wider; he will assume slow movements, etc. This is what the man will do more or less who desires to be pitied, or to look sorry when he is *not*, or to show his sympathy when he shakes hands with a mourning friend, or what a spoiled child will do who wishes to obtain what he wants. A father, a master who wishes to be feared and obeyed will feign anger, *i.e.* will imitate what he spontaneously does when he is really angry.]

The voluntary expression will be equivalent to the spontaneous one in so far as the reactions can be submitted to the command of the will. This power of voluntary imitation will therefore vary according to the reactions. Who can, using only direct means, voluntarily provoke in himself blushing, or paleness, or sweat, etc.? On the other hand, everybody—at least all normal people—can make with their arms, or hands, or legs, or muscles of the face, the movements they like. The power of voluntary imitation reaches its maximum in the striated muscles, and this is very important from the standpoint of the social action of expression since such movements are precisely the reactions which are the most important socially.

The complexity and delicacy of the voluntary muscular mimicry will depend upon the more or less advanced stage of the species in evolution, upon the relative functional independence of the muscles concerned,—an independence which is acquired in the course of evolution and constitutes the domain of the Will. But this power of voluntary imitation varies not only with the degree of perfection of the species: in exceptional cases it may also vary, within the same species, with the individual. As regards muscular reactions a few individuals will have extraordinary powers: they will be able to make voluntarily movements which usually are in man impossible to produce voluntarily, *e.g.* the contraction of the pupil, or even reflexly, *e.g.* movement of the ears. This personal power does not limit itself to movements only: certain people can, whenever they like, weep, blush, or become pale. It seems likely that will-power does not work here directly, but through the medium of representations: we can voluntarily evoke representations that will provoke or keep up in us such emotion. In this case the voluntary expression may be nearly identical with and nearly as complete as the spontaneous expression, for if the representations are intense they give rise to a veritable revival of the emotion and the emotional reactions are thus spontaneously called forth. There is yet another influence that can modify the field of the voluntary innervation: though within the same species the muscular and nervous connexions of most individuals are practically the same, nevertheless some will be capable only of imitating the expression of the 'coarser emotions,' others will also be capable of reproducing the finest *nuances* of the 'subtler emotions.' This seems to be due to mental differences: those who cannot imitate are those who cannot first experience.

Furthermore, G. Dumas quotes a third process: certain people, in order to produce phenomena upon which they have no voluntary power use indirect means of their own: such are the brief, jerky inspirations that one makes when desiring to weep and which act on the secretion of tears as a conditional reflex.

People who know best how to feign their emotions, such as the great actors, use these three processes:

(i) the direct one of voluntary innervation, which is limited by the nervous perfection of the species, and the physiological and mental power of the individuals;

(ii) the indirect one (psychological) which provokes the expression through the medium of representations;

(iii) the indirect one (physiological): conditional reflex.

We see now how the expression of the emotions can become a language, a medium of intercourse: it is a sign understood by those to whom it is addressed and who can in turn answer by the same sign: smile answers to smile, tears to tears.

IV.

But by dint of serving in social relationships the expression simplifies itself and becomes schematic, as always happens by habit. The formation of this schema, *i.e.* the elimination of certain reactions judged as unrepresentative and the conservation, even the strengthening, of certain others judged as characteristic, is made according to a certain collective ideal which is imposed by the social *milieu*, so that the schematic, socialised expression becomes different from the primitive and merely physiological expression. That is why, in so far as it is socialised, the expression is no longer understood by everybody but only by those who belong to the same social *milieu* as the subject. The expressions of joy, of sadness, of fear or of anger of a savage will be very well understood by us in so far as they are merely reflex; but if they are voluntary and more or less altered by social influences, we shall not be sure, say before the dance he is performing, whether he celebrates the joy of victory or prepares himself to fight by the symbolical, half-conventional mimicry of anger[1]. Here again an education is required since our previous personal experience is insufficient. We need more or less to be initiated into the meaning of the mimicry of politeness, mourning, joy, etc., which varies with the countries, and within the same country with the social *milieu*: the socialised schema of an expression is different according as it takes place on the stage or in real life, in an intellectual or popular *milieu*.

V.

Let us sum up the conditions that were required in order that the expression of the emotions might become a language:

To live in society.

To react spontaneously to an emotional shock in the same way as the beings with which we live.

To be capable of self-observation and of hetero-observation, both for him who is moved and sees the effect of his emotion in others, and for the observer who sees the expression and knows its significance.

To be capable of producing again, by voluntary innervation, or through the medium of representations, or by conditional reflex, the whole or only a part of the spontaneous emotional reactions, particularly the specific movements that are socially the most important reactions.

That such voluntarily provoked reactions be visible or audible for the spectators.

That there be a social agreement as regards the significance of the socialised expression.

All that precedes may be likewise applied to a theory of the origin of language. Language must have been formed by the continuation of this evolution of the expression of the emotions from the biological to the social; the emotional mimicry emptying itself more and more of its affective contents and becoming a mere symbol,—the language of gestures; on the other hand the emotional reactions specialising in cry, which itself became intelligent and more and more complicated,—spoken language.

---

[1] The rôle of this symbolical mimicry and specially of the mimetic dances is very important in the origin of both Art and Religion (cf. Jane Harrison, *Ancient Art and Ritual*). The well-known instance of professional mourners shows on another hand how far can go the socialisation of an emotional expression which, from an individual and spontaneous reaction, has developed into an institution. For other instances of this kind see M. Mauss: "L'expression obligatoire des sentiments (Rites oraux funéraires australiens)." *J. de Psychol.* May 1921.

(*Manuscript received* 1 *February* 1922.)

CRITICAL NOTICE

BY P. B. BALLARD.

Mental and Scholastic Tests. Report by the Education Officer submitting Three Memoranda by Mr CYRIL BURT, M.A., Psychologist. London County Council (King and Son). 1921. pp. xv + 432. 21*s.* net.

This long-expected book is characterized by the scholarly care and thoroughness which we have learned to look for in everything that Mr Burt produces. Its pages as we turn them over present at first sight some of the repellent qualities of a blue-book, but once we begin to read we realise that this is no ordinary 'report,' but a book of no little originality and charm—a book which sets forth with much lucidity and precision the results of a highly important piece of research. Mr Burt not only gives us a detailed account and searching criticism of the most popular system of mental tests—the Binet-Simon system—but also makes valuable additions of his own to our armoury of tests of intelligence and of scholastic attainments. And he repeatedly flashes light from an unexpected angle on the many problems that confront the teacher in his daily task.

Of the three memoranda which make up this volume the first two are wholly concerned with the Binet-Simon scale, the first with its practical use, the second with its theoretical validity; the third deals with scholastic tests. As a translation and revision of the Binet scale, the first memorandum challenges comparison with the American revisions of Goddard, Yerkes and Terman. The contrast is striking, for Mr Burt's attitude towards the tests is entirely different from that of the Americans. They regard the Binet system as substantially sound: it needs repairing in parts, and a slight remodelling to suit different nationalities, but the general plan and pattern of the scale cannot be bettered. Mr Burt on the other hand regards the Binet scale as a temporary makeshift. He says on p. 8:

> I am of opinion that, for exact and scientific purposes, an entirely new scale, or rather a new series of scales, must be constructed, built up afresh from the very foundation; to tamper or tinker with the old will not be sufficient. So ambitious a reconstruction, however, is not my present aim. It is an undertaking that would

> demand, not a single research, but a lifetime of researches. And, before the novelty could be accepted, an impartial criticism of the scales to be displaced should first be issued, based upon a fair and extended trial of those scales in their original authentic form—in a form where the results shall be comparable with both those of the first authors and those of subsequent investigators, and not in a modified and doctored version, half old, half new.

As a result of this policy we are here presented with a more comprehensive version of the Binet scale than has ever before been published. No detail of any importance is omitted. Whatever change in procedure or in content has been suggested by other reputable revisers is included in the author's comments.

It is the second memorandum, however, that will probably prove of greatest interest to the psychologist; for there Mr Burt discusses the theoretical implications, and criticizes the validity of the scale as a device for measuring intelligence. His criticisms are based mainly on statistical grounds—on the results actually obtained by applying the tests to a large number of London children, both normal and abnormal. Our acceptance of his conclusions will depend upon our acceptance of his ultimate court of appeal, and that again upon our acceptance of the concept of intelligence which that court consciously or unconsciously applies in arriving at its decisions. It is laid down on p. 199 that "there is no standard of comparison that can surpass or supersede the considered estimate of an observant teacher, working daily with the individual children over a period of several months or years." Again, "General intelligence was defined as inborn all-round mental ability." The teacher is the final judge, and the definition his statute. It is on the correlations found between the mental ages (as measured by the Binet-Simon tests) and the teachers' estimates that Mr Burt here bases his criticism of the general validity of the scale, and it is on the coefficients of colligation between success in the separate tests and intelligence as estimated by the teacher that he bases his judgment of the relative values of the individual tests. His criteria therefore demand a careful examination.

His definition, though perhaps as useful a working definition as could be devised, is, I submit, logically faulty. It implies a distinction between innate and acquired abilities which does not in point of fact exist. Every ability is both innate and acquired: it is innate as a potentiality—a neural or psychic predisposition; it is acquired as an actuality. Each requires the stimulus of a suitable environment to bring it into being. Although, however, there is no absolute distinction between innate and acquired ability there is obviously a relative distinction—a distinction that rests on two factors: first the facility with which the appropriate environment can rouse that ability into action (in other words the ease with which it can be trained), and secondly the degree of speciality in the environment itself. The shorter the period of training and the more general and casual the environment, the more inclined are we to call the ability innate; the longer the period of training and the more special and sophisticated the environment, the more disposed are we to call the ability acquired. Relatively to one another, the ability to see is innate and the ability to speak acquired; relatively to one another, the ability to speak is innate and the ability to read acquired. What the word 'innate' is probably intended to exclude is not all the effects of training but only those effects that come from habituation, from knowledge and from interest.

Another salient feature of Mr Burt's definition is that it makes the concept neutral: intelligence is general ability without emphasis on any particular mode or modes in which this ability manifests itself. This indeed is the *a posteriori* notion of intelligence at which we arrive after an examination of the American group tests. If the American army tests and their numerous progeny measure intelligence, then intelligence means mental ability shown in any direction—in facility of apprehension, retentiveness of facts and speed of execution no less than in the power to solve the most abstruse problems. Here, it seems to me, we strike the fundamental difference between the English school of mental testers and the American. The Americans tend to accept mental tests, whether they are modelled on the Binet scale or on the American army scale, as their own evidence. They measure something and that something is labelled 'Intelligence.' The experimenters calculate the correlation between one series of tests and another, find the correlation fairly high, and are satisfied. When they find the correlation between the results of the tests and the estimates of an independent observer to be low they question the judgment of the observer. They regard the tests as the more trustworthy.

Mr Burt, as representing the English school, adopts a different plan. He regards the current concept of intelligence as the important criterion, by comparison with which the tests themselves must stand or fall. While the Americans try to mould the concept to the test, Mr Burt tries to mould the tests to the concept. But the current concept as it exists in the mind of either the psychologist or the layman is not, I take it, the neutral concept that the wording of Mr Burt's definition would lead us to believe. It is as a matter of fact *biassed towards the higher mental functions*. We set more value on the mental ability displayed in reasoning out a problem than in repeating numbers because the former brings more thought into play than the latter. The consequence of this bias is that when tests are evaluated on the basis of their conformity with the current concept of intelligence, those tests come out best which demand from the subject the greatest amount of thought. We are not therefore surprised to find that the results of all Mr Burt's investigations point to the same goal: they all end in placing a premium on the higher mental processes, and a discount on the lower. If his methods are sound, his conclusions are inevitable. And to me his methods seem preeminently sane and sound. Those who have followed the development of mental testing in other countries realise that unless the aim is clear, unless the testing is anchored, so to speak, to some sort of stable concept, it tends to drift further and further away from the original intent and purpose.

The animadversions which Mr Burt is ultimately forced to make on the Binet tests are too numerous to quote. Indeed it would be hopeless to try to give an adequate account of the material and the methods of this book—the many and various group tests of intelligence and of scholastic attainments, the illuminating comments on those tests and their results, and above all the scrupulous attention paid to the *technique* of testing. One gets the impression of a mind in constant quest of an almost impossible ideal of exactitude in the measurement of intelligence, impatient of all expedients that fall short of that ideal, yet forced in the interests of present needs to use those expedients, pending the discovery of others more precise and more perfect.

The book makes pleasant reading; for the author is among the few who write with the precision of the man of science and the charm of the man of letters.

PUBLICATIONS RECENTLY RECEIVED

Instinct and the Unconscious. By Dr W. H. R. Rivers. Cambridge: University Press. Second Edition, revised. 1922. Pp. viii + 277. 16*s*. net.

Two appendices on *Psychology and the War* and on *The Instinct of Acquisition* have been added to this edition. These have already appeared in *Scribner's Magazine* and in *Psyche* respectively. Some slight changes have been incorporated in regard to the topic of dissociation—the result of Dr T. W. Mitchell's criticisms. Otherwise the book is unaltered.

The Psycho-analytic Study of the Family. By J. C. Flügel. The International Psycho-analytical Press. 1921. Pp. x + 259. 10*s*. 6*d*. net.

A detailed examination of this interesting book has already appeared in the *Medical Section* of *This Journal*. Although a notice of similar length is again needless, attention may well be drawn here to the value of Mr Flügel's work from other standpoints than that of medical psychology. He disclaims too modestly any originality: he terms it "a work of compilation" and says that "with the exception of small contributions and suggestions upon special points, in the last few chapters alone does there exist anything that has not already found a place in the literature dealing with the subject." That literature is by no means solely confined to the psycho-analytic kind. The views of Westermarck, Wundt, Durkheim, McLennan and Herbert Spencer receive attention. Such subjects as incestuous affection, ideas of birth, the influences of the family in social development and religion are carefully treated, and the final chapters deal with the ethical and practical applications of the conclusions reached in the course of this thoughtful work.

The History of Human Marriage. By Professor Edward Westermarck. London: Macmillan and Co. 1921. Fifth Edition. Three volumes. £4. 4*s*. net.

In this new edition of Dr Westermarck's well-known work, first published in 1891, "the book has been rewritten throughout to such an extent that very few sentences of the earlier editions have remained unchanged....In short, it is a new work much rather than a new edition" (Preface). Thus the subject of polyandry, instead of being treated in a few pages, is extended to two chapters (XXIX and XXX); group marriage has now a chapter (XXXI) to itself; so have religious celibacy and sexual modesty (XI and XII) respectively; the *jus primae noctis*, religious prostitution and the lending or exchange of wives cover 69 in place of 9 pages. To the subject of marriage rites a more than twelve-fold increase of space (chs. XXIV–XXVI) is devoted—notwithstanding the author's statement that while he believes more than ever in the influence of magical beliefs on marriage rites, he believes less in the value of these rites for the study of the earlier forms of marriage.

But despite these large additions, mainly due to the enormous accumulation of illustrations, the work remains substantially the same; neither its general characters and methods, nor its fundamental ideas have been altered. Dr Westermarck's object still is to prove by the 'comparative method' that sexual promiscuity has never been a "general stage in the social history of mankind" (I. 336). He still holds that the classificatory system of relationships has not necessarily arisen through group marriage or sexual communism, that exogamy is due to an instinctive aversion from sexual relations with those too intimate, and that marriage has always implied marital and paternal protection, *i.e.* the institution of the family.

The new pages devoted to female coyness, sexual attraction, and secondary sexual characters generally are somewhat disappointing in psychological interest. The views of the psycho-analysts, especially in regard to incest, receive barely passing mention. A more important chapter for the psychologist is that on method (ch. I) which has been specially written for the present edition. Here Dr Westermarck vigorously combats Dr Rivers's distrust in our capacity to arrive at the motives by which social actions are determined. "There is no more depressing and apparently hopeless task," Dr Rivers has written, "than that of trying to discover why people perform rites and ceremonies and conform to the social customs of their community." He has also pointed out that "no mental states are more difficult to introspect than emotions and sentiments, to say nothing of instincts," and that it is these, not intellectual motives, which generally determine social conduct. Dr Westermarck retorts that, on this principle, we should "carefully refrain from assuming, for example, that courtship and marriage have anything to do with the sexual instinct, that the retaliation of adultery springs from jealousy and revenge, that the secrecy observed in the performance of the sexual functions is connected with sexual modesty" (I. 9). The effectiveness of this reply hardly needs discussion; and the psychologically trained reader can hardly peruse Dr Westermarck's work without feeling that he has too often built unwarrantably on the supposition that the attitude of human beings to given conditions must always have been in the remote past the same as it is to-day. Wedded to the comparative method, he is bound to defend it (although he admits (I. 13) his growing distrust of ethnographical evidence and (I. 15) its interpretation). He still quotes with approval Tyler's confidence in the account of a native ceremony or myth, where the same account is described to us at different times by two quite independent observers. No doubt, too, our author would agree with the celebrated statistician who maintained that the errors of rough measurement are 'in the long run' cancelled by obtaining data in sufficient number. The fallacy of such views has been exposed by the psychologist: the material collected by several observers may all be biassed in the same direction. Still, however limited in value, the comparative method has proved of great service to ethnology. It owes its development to the labours of Tyler and Frazer in this country, of whom Dr Westermarck has proved himself a loyal and worthy follower.

Abzählende Methoden und ihre Verwendung in der psychologischen Statistik. Von Dr OTTO LIPMANN. Leipzig: J. A. Barth. 1921. S. 78. Mk. 16.

This work deals with statistical methods in which the median and quartiles play the most important part. The critical pages are those dealing with the calculation of correlations (or co-ordinations, as the author terms them when ranks are used, as here). Three methods are described, of which one is the following, which we here explain by a small example rather than in symbols:

Imagine two rank orders (i) and (ii) to be correlated. Their differences are formed in the usual way, regardless of sign.

Rank-Orders		
(i)	(ii)	Differences
1	2	1
2	5	3
3	1	2
4	3	1
5	4	1
6	6	0
7	8	1
8	10	2
9	7	2
10	9	1
11	11	0

These differences are then themselves arranged in order thus: 0, 0, 1, 1, 1, 1, 1, 2, 2, 2, 3. Their quartiles and median are 1 : 1 : 2, where the lower quartile is given first, median second, and upper quartile last. These are brought up to a standard size by multiplying by $\frac{100}{n}$, n in our case being 11. This gives 9 : 9 : 18. These have now to be compared with the corresponding values for random change of rank (no correlation) which are 13 : 29 : 50.

The comparison is given numerical value thus

$$\frac{13-9}{13} : \frac{29-9}{29} : \frac{50-18}{50} \qquad \text{......(1)}$$

or

$$0{\cdot}30 : 0{\cdot}34 : 0{\cdot}64.$$

Here 0·34 is the coefficient of 'co-ordination.' The other numbers which flank it indicate its interquartile range, they define its probable error, as it were.

Now the formula $\rho = 1 - \frac{6\Sigma(d^2)}{n(n^2-1)}$ which is in general use for such cases as the above gives a value 0·87, which corresponds to $r = 0{\cdot}88$.

Lest this great difference between Lipmann's method and the Spearman-Pearson formula should be due to the small number of cases, here is an instance worked out, one in which $n = 50$:

$$\text{Lipmann's 'co-ordination'} = 0{\cdot}41,$$

$$r = 2 \sin \frac{\pi\rho}{6} = 0{\cdot}72.$$

It would seem therefore that this first 'co-ordination' method (for which no mathematical deduction is given) does not measure what we ordinarily call 'correlation.' The idea of using the median difference is probably sound enough as far as it goes, but the formula (1) above is defective.

Dr Lipmann gives two other methods of calculating 'co-ordination,' and considers the third method the most exact. It cannot be explained in the space here available, but it does not appear to be any more satisfactorily grounded than the first method.

Peetickay—An Essay Towards the Abolition of Spelling. By WILFRED PERRETT. Cambridge: W. Heffer and Sons, Ltd. 1920. Pp. 96. 6*s*. net.

It is refreshing to meet a book on spelling reform which is both delightful to read and which arouses none of those antagonisms so frequently the result of the suggestions made by the Simplified Spelling Society. The urgency of the need for spelling reform has long been felt: the difficulty has been the absence of a suitable scheme. Such a scheme 'Peetickay' supplies in the form of a new but simple rational vowel notation together with some changes in the existing consonants, so giving one symbol only for each sound common in spoken English.

'Peetickay' is easy to learn, easy to read. It avoids the need for knowing how to spell: its writing is simple and clear.

These points should claim for it the consideration at least of every one at all interested in the question of the reform of spelling with all that this involves.

La Perception Visuelle de la Profondeur. By LOUIS LAVELLE. Strasbourg: Commission des Publications de la Faculté des Lettres, Palais de l'Université. 1921. 2*s*. net.

La Dialectique du Monde Sensible. By LOUIS LAVELLE. Strasbourg: Commission des Publications de la Faculté des Lettres, Palais de l'Université. 1921. Pp. xlv + 228. 6*s*. net.

The larger book starts out from the concept of Pure Being, and 'deduces' in orderly succession the Given, Space, Time, Movement, Force and Quality; the latter neatly and symmetrically in two series of five internal and five external senses.

The treatise on the Visual Perception of Depth gives this psychological problem the specific benefit of the same principles and method. As, however, the auspices of the Divine Intelligence are claimed for these, Psychology, one fears, must plod along without them.

Health Education and the Nutrition Class. By JEAN LEE HUNT, BUFORD J. JOHNSON and EDITH M. LINCOLN. New York: E. P. Dutton and Co. 1921. Pp. xiv + 281.

This is a report of an interesting series of educational experiments carried on in a Manhattan Public School during 1918–1920, with the aim of remedying malnutrition in the school population. The book is primarily of importance for the student of hygiene and physiology, but certain of its data and results are of considerable interest to the psychologist, with regard to the relation between mental ability and nutritional conditions. In the first set of children dealt with, one group of underweight children was drawn from the "Terman" class, *i.e.* the class of supernormal children, with intelligence quotients well above 125. In the Terman class as a whole, which included boys and girls with an age range from 10–12 years, there was a smaller percentage of children 7 per cent. or more overweight, and a larger percentage of children 7 per cent. or more underweight, than in any other class studied in the same period (these covering children from the first, fifth, sixth and seventh grades, and from the special open-air class).

The "Terman" group was found to respond to remedial treatment better than any other. This was probably due in part to the fact that an attempt to win the intelligent co-operation of the children themselves was an integral method of the treatment; and in part to the probable fact that the underweight of these mentally superior children is due to an increased metabolism. In a later experiment, the method using a Control Group was adopted. A comparison of the intelligence ratings of the members of the Control and the Nutrition Groups suggests that the under-nourished children distribute themselves quite similarly to the children of normal height-weight-age index, but that the under-weight group has a slightly higher average of intelligence. And a comparison of the gain in weight under treatment made by those having a high intelligence quotient with the improvement among those with a low I.Q. confirms the early result with the children from the Terman class, there being a significant difference in favour of the more intelligent.

Psychoanalysis of the Reformer. By JOEL RINALDO. New York: Lee Publishing Co. 1921. Pp. 137.

An amusing book, alike in the vigour and extravagance of its style, and in its content and polemical purpose. It is mainly concerned with extreme Prohibitionists, of whom he writes—"They show marked symptoms of Anæsthesia, Amnesia, Motor Disturbances, Abulia, Phobia, Fixed-Idea, and Ego-Mania; and such a co-ordination of symptoms can argue nothing but a chronic abnormal psychic condition, definable as meddling hysteria." It is little wonder that the author calls for the psycho-analysis of all reformers.

Otis General Intelligence Examination. By A. S. OTIS. New York: World Book Co. Specimen set, 10 cents postpaid.

This is a test designed specially for use in commercial institutions, where applicants suitable for clerical and executive positions are to be selected. The pamphlet contains the 75 questions of the test, with manual of directions and examiner's key. In essentials the test has much in common with the Alpha Army Test, but is so constructed that it can be given either as an individual or a group test, and that it can be administered and scored by an ordinary clerk.

On the principle that speed associated with accuracy is worth more than speed accompanying carelessness, a special schedule of credits for speed is provided, The percentile rank for each score among clerks already tested, and among privates in the U.S. Army, is given, and the author requests that the results of examination be sent to him, in order that more reliable percentile rank tables may be constructed, In consideration of this co-operation, the latest tables compiled will be sent to investigators.

Schriften zur Anpassungstheorie des Empfindungsvorganges. By Professor JULIUS PIKLER. 1922. Leipzig: J. A. Barth. Part 4. Theorie der Empfindungsqualität als Abbildes des Reizes. Pp. 107. M. 20.

In this, the fourth of a series of monographs intended to establish his "Adaptive Theory of Sensory Processes," Professor Pikler continues to point out to experimental psychologists the error of their ways. To the current non-representative theory of sensation he opposes the concept of equivalence, arising out of the active adjustment of the organism to the stimulating force, by means of the development of a like counter force, expressed in sensations which reproduce the stimulus. The statement of the theory is able and attractive; but Professor Pikler can scarcely expect experimental psychologists to mend their ways, with the object lesson of his conclusions before them. Identity between the primary qualities of the different senses is alleged; and in the four-fold schema laid down for touch, taste, smell, colour and space-forms, yellow is salt and an acute angle; tickling is green and curved; a broken-off acute angle is red and putrescent, etc.! The experimental evidence is negligible.

Die Bewusstseins-Vorgänge bei Suggestion und Hypnose. By Professor MAX KAUFFMANN. Halle: Carl Marhold. 1921. M. 4.40. Pp. 36.

A very popular exposition of suggestion and hypnotism, in relation to the levels of consciousness. There is an attempt to show the relations of these levels in diagrammatic form.

Second Annual Report of the Industrial Fatigue Research Board. To September 1921. H.M. Stationery Office. 1922. Pp. 65. 1*s.* 6*d.* net.

This report is a valuable index to the condition and progress of industrial psychology in England, and it is so clearly and simply written that it forms an excellent introduction to the subject for any intelligent employer or workman who wants to know what the psychologist and physiologist has to offer him. Its value to the professional student lies not only in its summary of the detailed methods and results of the various researches which have been carried out by the Board's investigators, but also and mainly in the clear and systematic survey of the problems of psychology and physiology in industry, and of their inter-relations. These are set out and discussed with a detailed insight which the work of the Board itself has alone made possible.

The Foundations of Psychology. By Dr JARED SPARKS MOORE. Princeton: Princeton University Press. London: Humphrey Milford. 1921. Pp. xix + 238. 12*s.* 6*d.* net.

The author complains that at present we find "not one science of psychology but many—structural psychologies, functional psychologies, behaviourist psychologies and others, each one claiming to be the truly scientific psychology, but having aims and ideals inharmonious with all the rest." He essays a systematic survey of the essential elements in each of these rival points of view, in the hope of achieving a genuine synthesis. The work is in three parts; the first discusses the various conceptions of psychology, the second delimits the field of scientific psychology, its relations to metaphysics on the one hand, and to the physical and biological sciences on the other, the third considers the postulates necessary for a scientific psychology, with special attention to the problems of parallelism, psychical causation, and the subconscious. The survey is carried out in a scholarly and thorough way, with full references and quotations.

Here and Now Story Book. By LUCY SPRAGUE MITCHELL. New York: E. P. Dutton and Co. Pp. xii + 360.

This is an interesting and valuable contribution to the problem of children's literature. The author rejects our common adult conventions as to what is 'childlike,' and goes direct to the spontaneous creations of little children themselves for insight into the meaning of the story to the child's mind.

The Hidden Self. By H. Ernest Hunt. London: W. Rider and Son. 1921. Pp. 172. 4*s*. 6*d*. net.

An entirely superficial account of the 'new psychology,' with a strong didactic character. The author makes the common mistake of confusing external deprivation and 'repressive' methods of education with psychical repression in its technical sense.

The Psychology of Learning. By William Henry Pyle. Baltimore: Warwick and York. 1921. Pp. 308.

As a summary of the experimental studies of the learning processes, and a review of the evidence with regard to the problems of economical learning, the measurement of learning capacity, differences in learning capacity, transference, interference and fatigue in learning, this is a very useful and reliable volume. The presentation is clear and readable, and many graphs are supplied. But on the theoretical side, and on the broader issues, the book is weak.

Handschrift und Charakter. Von Dr Ludwig Klages. Dritte und Vierte unveränderte Auflage. Leipzig: J. A. Barth. 1921. S. xi + 254.

Handschriftenproben zu Handschrift und Charakter. Von Dr Ludwig Klages. Leipzig: J. A. Barth. 1921.

To anyone who is prepared to take this subject very seriously no better monograph that the study of Dr Klages could be recommended. Practically all the possible features of handwriting are separately and carefully considered, and their development discussed. Then come sections on the main groups of individual differences of temperament, and the possible connexion of the different features of writing with these are indicated. Very excellent reproductions of different types of handwriting are included in the separate pamphlet. On the whole the analysis of writing is better than that of character. The latter is somewhat formal, and the traits distinguished are almost all themselves very complex. There is a short note on the literature of the subject.

NOTES ON RECENT PERIODICALS.

Arbeiten zur Entwicklungspsychologie.

Viertes Heft.

Variants of literary motives. I. *Hebbels' Agnes Bernauer*; II. *The legends of the early fathers.* (Bruno Golz.)

This is an extremely interesting and well-presented monograph. If anything, the study of Freidrich Hebbels' drama is to be preferred to the consideration of the legend of the early fathers which forms the second, and longer, part of the treatise. But this is only because the former is perhaps a trifle more concrete. The story of how varied versions have dealt with transformation elements in the legend is particularly interesting.

Fünftes Heft, 1921.

The treatment of economics in the philosophical thought of the 19th century. (Hans Freyer.)

Dr Freyer's monograph is a very careful study of the underlying ideas of the various schools of economic thought which flourished during the last century, with considerable reference to their historical setting. Although the survey presented is not very directly psychological in its point of view, it may be recommended as well worthy of study by anyone who is concerned to work out the psychological character of modern economic movements. The discussions in the last two chapters particularly —on *Technik und Seele* and *Die Überwindung des kapitalistischen Menschen*—should prove of great interest and value to the student of social psychology.

Arbeiten zur angewandten Psychiatrie.

Bd IV, 1921.

The psychology and psychopathology of the history of the present time. (Gaston Roffenstein.)

This is an exceedingly interesting study in contemporary social psychology. The author is on the whole rather more interested in abnormal than in normal social behaviour, and his monograph would gain somewhat if he occasionally broke more completely away from his many authorities, and attempted more really first-hand investigation. Still as a study of modern class movements of a largely violent character it should prove stimulating to further research.

Schriften zur Anspassungstheorie des Empfindungsvorganges.

Drittes Heft, 1920.

Theory of sensory intensities and especially of Weber's law. (Julius Pikler.)

Professor Pikler here continues his general studies concerning the 'adaptive' character of sensory experiences, dealing with the vexed question of intensity. His brief essay, which is vigorous rather than convincing, is divided into two parts. The first disqusses a general theory of sensation intensity, treating the latter as a kind of balancing process, or equivalent psychical response to a stimulating force. The second, on Weber's law, is wholly general.

Columbia University Contributions to Philosophy and Psychology.

Vol. XXVIII, No. 2.

The Morphologic Aspect of Intelligence. (Sante Naccarati.)

Dr Naccarati rejects any attempt to find a correlation between intelligence and any simple physical trait such as height, weight or cephalic index, and insists that any physical trait likely to be correlative with intelligence must be compounded of several anthropometric traits. In his experimental study, he found a positive correlation between the height-weight index and intelligence, but this was higher when the morphologic index of De Giovanni and Viola was used, *i.e.* the ratio of length of limbs to volume of trunk. Of the three morphologic types, the microsplanchnic gives the more intelligent units than the normo- and the macrosplanchnic.

Beiträge zur Analyse der Gesichtswarhnehmungen.

No. 6, 1922.

On the influence of the interval on the reproduction of letters. (G. V. Wattensleben.)

It had been concluded by Finzi that in a range of intervals from 2″ to 30″, the optimum was reached at 4″. Wartensleben's investigation shows that this conclusion is unwarranted and that there is no determinate optimum, but, corresponding to the variety of factors present, a quite diffuse distribution over the range of intervals 0″–15″.

Experiments on memory-traces. (Hans Henning.)

Ranschburg found that in six-figure sets, homogeneity (the type *abcxxd* or *abxcxd*) was an important source of error in reproduction. From this he attempted to derive a far-reaching neuro-psychological law that all similar elements tend to coalesce. Aall confirmed the source of error, but put it down, not to coalescence of similar stimuli, but to interference with the perseveration factors. Schulz, using figures and colours, obtained better results from homogeneous than from heterogeneous series. The question was thus undecided. Henning's experiments, covering the whole ground, appear to refute Ranschburg decisively, and to show that the memory traces are determining, and may react to homogeneous stimuli in a number of ways, of which all the previous results were specific cases.

Experimental contribution to the psychology of reading. (Julius Wagner.)

Erdman and Dodge found that there was a large quantitative difference between the recognition of familiar words and of series of unmeaning letters. They explained this effect as produced by the rough total form of the familiar words; Zeitler set up the theory of determining letters. But meanwhile the experiments of Erdman and Dodge have been criticised, and Wagner's tests confirm the justice of these criticisms. Provided that attention is directed on the whole field, and not only on the fixation point, it turns out that the "large quantitative difference" almost disappears.

Investigations into the certainty of testimony. (Georg Ries.)

Starting from G. E. Mueller's examination of the conditions of subjective certainty, Ries made four series of experiments, covering (1) letters and figures, (2) simple geometrical forms and easy drawings, (3) significant words, singly or in pairs, (4) odours. Mueller's statements are mainly confirmed, the view that the exclusiveness of a representation is a main condition being corroborated. Other contributory factors are quickness, clearness, persistency and detail; and also associative connection (whether rote or logical) with other representations. The small percentage of statements subjectively certain but in fact erroneous are seen to be due to the way in which the ideational factors gain the upper hand.

The judgment of recognition. (F. Schumann.)

The calling up of an auditory-motor image is a part of the process of recognition. If this is felt to belong together with the presented material, recognition occurs. In the case of smells in particular, the effect of the name, whether hit upon by the subject of the experiment or supplied to him, is frequently well marked; it calls up an associated olfactory image, and an explicit comparison follows, resulting in a judgment of recognition. This comparison seems to be necessary, although it may be so immediate as scarcely to be conscious. On the other hand, it may not be sufficient; the consciousness of congruence to which it leads may involve a specific psychical component not yet identified.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETINGS.

May 13, 1922. Effects on Mental Process of Oxygen Deprivation, by J. P. Lowson.

The Graphical Treatment of Data obtained by the Methods of Mean Error, by R. J. Bartlett.

SECTIONAL MEETINGS.

Education Section.

May 9, 1922. An Analysis of Literary Appreciation, by Olive A. Wheeler.

Medical Section.

June 28, 1922. The Nomenclature of Minor Mental Disorders, by Millais Culpin.

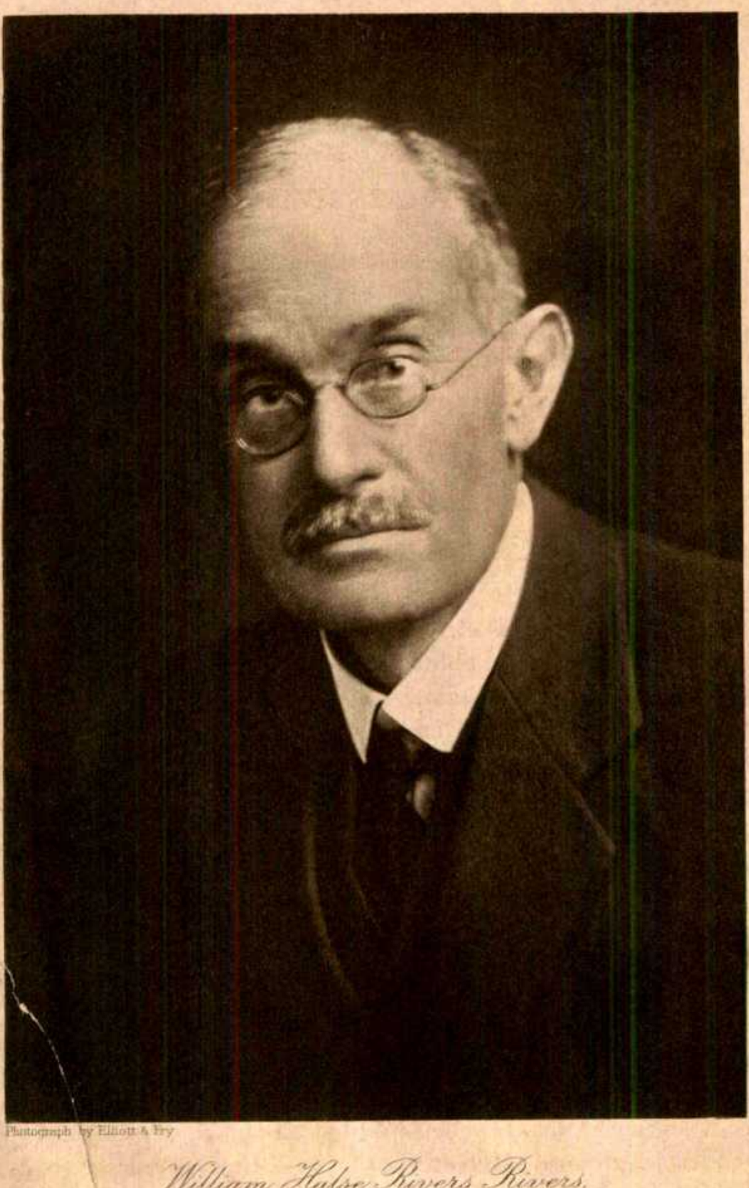
Aesthetics Section.

May 17, 1922. The Future Work of the Section, by E. Bullough.

JOINT MEETING.

Education and Industrial Sections.

June 13, 1922. Vocational Tests, by G. H. Miles.



Photograph by Elliott & Fry

William Hulse Rivers Rivers.
1864-1922.

VOLUME XIII OCTOBER, 1922 PART 2

THE RELATIONS OF COMPLEX AND SENTIMENT. I.[1]

BY THE LATE W. H. R. RIVERS.

IN considering the relation to one another of the concepts denoted by 'complex' and 'sentiment,' it is necessary to consider briefly the history of the two terms which in spite of its recency is already far from simple.

The term 'complex' was first employed by Jung[2] but without any attempt to define it exactly. He distinguished between normal and dissociated complexes, but both he and others came to use the term with a pathological implication. It came to denote a system which has become, or tends to become, separated from the rest of the mental structure or personality and by its domination of the mind has destroyed, or tends to destroy, the unity which is essential to mental health.

Bernard Hart[3] extended the meaning of the term by using it for any "emotionally toned system of ideas" which determines conscious behaviour, taking the hobby and political bias as his special examples.

Tansley[4] has extended the meaning of the term still further and applies it to a system of associated mental elements the stimulation of any one of which tends to call the rest into consciousness through the medium of their common affect. The connotation thus given to the term is so wide as to destroy any value which the term, as used by Jung and Hart, possesses. Except in those cases in which mental states are wholly free from affective accompaniment, every act of perception, association, memory or other mental process would depend on the activity of a complex in the sense in which Tansley uses the term. Hart, and still more Tansley, by giving 'complex' so wide a connotation have gone far to deprive the term of any value as a serviceable instrument of thought.

Moreover, in the broad sense in which Hart uses the term it becomes almost, if not quite, identical with the 'sentiment' of recent British psychology. A sentiment may best be defined as an organized system of emotional tendencies grouped about an object, the distinction from affect lying in the emphasis laid on tendency in place of activity, and the features of mental life cited by Hart as examples of the complex answer to this definition of the sentiment. The place of the hobby in the mental life of a person depends on the existence in the mind of the person of a group of emotional tendencies centering round the scientific, literary or other special kind of activity which forms the essential feature of the hobby. Again, political bias depends on the presence of an organized system of emotional tendencies centering round the political party to which a person belongs, or round some group of political ideas which have not yet become the watchword of a party. If we are to use the term 'complex' in Hart's sense it is unnecessary, for psychology already possesses a term admirably adapted to the concept which needs denotation.

---

[1] A contribution to the Symposium prepared for the Meeting of the British Psychological Society in Manchester, July, 1922.

[2] *The Psychology of Dementia Praecox*, New York, 1909, 36 *et. seq.*

[3] *The Psychology of Insanity*, Cambridge, 1912, 61.

[4] *The New Psychology*, London, 1920, 49.

In his original use of the term it seems clear that Jung had especially in his mind the process of dissociation, but in my opinion we shall be making the term of more value if we make suppression[1] the outstanding element in its meaning, this process being absent or taking only a secondary place in the production of the sentiment.

In considering the part which the process of suppression is to take in the formation of our concept of the complex, it will be well briefly to summarise the place it occupies in mental development and its relation to the alternative process of fusion[2]. According to the point of view from which I am approaching the subject, the great problem which had to be solved in the process of mental development was how mental processes and activities should be treated when, being adapted to one kind of existence or one phase of evolution, they had to be modified to meet circumstances of a different kind. Two main alternatives were open. The earlier activity with its products could be suppressed and treated so as to make them inaccessible to consciousness when incompatible with later developments; or they could be utilised by fusing them, or parts of them, with the products of later development. In the first case the suppressed activity will maintain its individuality in so far as it continues to exist; in the second case, it loses its individuality and becomes merged in the product of the process of fusion so that its nature can only be revealed by a special process of analysis. The processes of suppression and fusion represent two widely different, and to a large extent antithetical, modes of treating experience, and I suggest that we can give to the terms 'complex' and 'sentiment' their greatest value in psychology and psycho-pathology if we make suppression the keynote of the complex while fusion forms the essence of the process upon which depends the formation of a sentiment. According to this usage the concept of 'complex' is closely linked with that of the unconscious. The term is appropriate to those cases in which experience shut off from direct access to consciousness is nevertheless capable of influencing conduct or of producing changes in consciousness, the underlying conditions of which cannot be reached by the ordinary process of introspection. Characteristic examples of a complex would be found in the suppressed experience upon the activity of which depends a phobia or the compulsive act or thought of a compulsion-neurosis. The complex is usually of a relatively simple kind and may consist only of some body of experience which has been suppressed, though more usually the suppressed experience takes with it into the unconscious experience of other kinds with which it has been associated.

---

[1] I use this term with the significance attached to it in my book *Instinct and the Unconscious*, Cambridge, 2nd edit. 1921, 17. It corresponds in some measure with the 'repression' of the psycho-analytic school, but as used by that school this term has come to have so confused a meaning and covers processes so different from one another that it has little value as a scientific term.

[2] See *Instinct and the Unconscious*, 32.

Such a feature of mental life as political bias, on the other hand, is the product of large and varied experience from childhood, youth and adult life, much of which is readily accessible to consciousness, though much may lie on its threshold, while other elements may involve experience which has been suppressed. Into the process of fusion which forms the essential mechanism of production of the sentiment there enter experiences of many kinds, some of which may be closely linked with the content of the unconscious and have only escaped from becoming part of its content because they have been utilised in the process of fusion in place of being suppressed. The bias is a highly complex product of a process of fusion between trends of thought and conduct which may sometimes be influenced by unconscious experience, just as any other mental process may be so influenced. Prominent among the elements of the bias, however, it is often possible to distinguish the influence of relatives, friends and teachers which has never been the subject either of suppression or repression.

It is an interesting example of Jung's failure to realise the nature of the mental product he was denoting that he should have chosen the term 'complex.' If he had called it 'simplex' he would have been far nearer the truth, but it is unfortunate that the criterion of simplicity or complexity should have been made the basis of nomenclature. The two outstanding features of the complex are its relation to suppression and its affective importance and if, as seems likely, the widespread misuse of the term makes it necessary to invent another, it is to be hoped that this new term will bear a relation to these two characters.

Another feature which serves to distinguish complex from sentiment is the greatly different degree in which they are capable of modification. The complex, once having come into being through the process of suppression, is highly constant. It may persist for years, even for life, unchanged and unchanging. It may disappear when brought into direct relation with the consciousness from which it has been separated, and in this respect shares with other features of the unconscious and its mechanisms the 'all-or-none' character which I have ascribed to them. It either exists or disappears. It is not capable of the variations of which the experience of the conscious life is continually the subject. Months or years may pass without the complex revealing its presence, and then it may manifest its influence by some profound and far-reaching disturbance of the mental life.

The sentiment, on the other hand, is not only capable of modification, but this capacity is frequently utilised in experience. The sentiment is a fluid product which in an active life is always changing through the influence of new experience. Thus, the pronounced conservative bias of the schoolboy brought up in the manor house or the country parsonage may change insensibly into a sentiment of a very different kind through the influence of education, the new surroundings of the adult, and intercourse with persons from many different callings and ranks of life to which the boy had been a stranger. To use the language adopted in my book, *Instinct and the Unconscious*, the complex is a characteristic example of a protopathic product of the mental life, while the sentiment, through its capacity for graduated activity and modification, can be assigned with confidence to the epicritic category.

A third distinction between the complex and the sentiment, a distinction perhaps more important than either of the others, is that the sentiment is a necessary and constant feature of the normal mental life. Most of our sentiments come into action daily and influence the behaviour of every moment of the life of every day. The complex, on the other hand, may lie dormant for long periods of time and is in no way necessary to the harmony and fitness of the mental life. Indeed, it is a question whether its activity does not always involve a lack of harmony or a failure of adaptation to the circumstances with which the organism is called upon to deal.

This raises the problem to what extent the complex can be regarded as having always a pathological implication. There seems to be little doubt that, as originally used by Jung, it had to a large extent this connotation, and if morbidity be made a fourth character of the complex and means of distinguishing it from the sentiment, the usefulness of the term would be enhanced.

It is convenient that morbid psychology should have, at any rate in some cases, its own terms, just as it undoubtedly has its own concepts. In psychology, quite as much as in physiology, the morbid shades off insensibly into the normal, and cases constantly present themselves where it is a task of the utmost difficulty to state the exact position of a mental product between the two ends of the series of which the healthy and the morbid form the limits. Nothing but confusion, however, can result from the inclusion under one heading of definitely pathological states and states so normal and so necessary as the sentiments. I believe that we shall be making the two terms 'complex' and 'sentiment' of most service to psychology if among their other distinctions we include that between the healthy and the morbid, always recognising the existence of transitions, and making it our task to evaluate the two aspects in cases intermediate between the definitely healthy and the definitely morbid.

Moreover, just as cases occur in which a mental state or process occupies an intermediate position in respect of healthiness and morbidity, so will there be many states which cannot at once be classed with complex or sentiment since they are intermediate between the two. Thus, while the hobby in general may be regarded as a perfectly healthy and normal feature of the mental life, it may sometimes acquire a character which can hardly be regarded as other than morbid. When a collector becomes so absorbed in his hobby that he steals an object needed to make his collection more complete, we have a case in which the hobby has acquired a morbid character. Moreover, such a case gives us a further clue to the nature of the distinction between complex and sentiment, for there is little doubt that this approach of the sentiment to the complex has arisen through the activity of an instinct, the instinct of acquisition, which is normally modified owing to its incompatibility in its crude form with social sentiments and traditions[1].

Again, when political bias has reached such an acme of development that the great majority of an electorate vote for a notorious swindler simply because he has been chosen to be the representative of a party, we are dealing with an outgrowth of a sentiment which is only not regarded as morbid because we are accustomed to think of disease as a feature of the individual and not of the social life. There is little doubt that in such development and in the acquirement of such a morbid character, political bias owes much to the inclusion in its formation of activities closely allied to, if not actually themselves, complexes. It is noteworthy that in such cases as I have cited, the hobby or the political bias have other characters in which they approach the complex. It is probable that the theft of a bookplate or a woodcut by a collector is rarely a premeditated act, the nature of which is clearly discriminated, but has the impulsive character normally associated with the activity of a complex, while those who vote for a notorious scoundrel are not those who have developed their political sentiment, but are people whose political behaviour resembles so many other examples of collective behaviour in having the explosive and 'all-or-none' character of instinct.

---

[1] *Psyche*, 1921, vol. II, 100; see also *Instinct and the Unconscious*, 2nd ed. 1922, 248.

It is noteworthy that the sentiments chosen by Bernard Hart to illustrate his concept of the complex should be two which so often lead to behaviour resembling in many respects that dependent on the activity of the complex in the stricter sense which I have given to it in this opening paper of our Symposium. But I believe that even in that extreme form in which they can be regarded as morbid, the hobby and the political bias differ from the complex in that they are not simple products of the process of suppression, but far more complex products into the complicated tissue of which there have been woven elements in which the process of suppression has taken part. While the hobby or the political bias may sometimes resemble the complex in leading to morbid behaviour, or at least to behaviour having an explosive and ungraduated character, there still remains the fundamental distinction between the complex and the sentiment that one is wholly the product of suppression, while the other is the highly complicated product of a process of fusion in which suppression only plays a part in that it enters into the make-up of some of the elements upon which the process of fusion has acted. Both the hobby and the political bias belong to the category of sentiment because of their highly complex character, because of the high degree in which they possess the character of complexity which Jung has so unfortunately and inappropriately chosen as a means of denoting suppressed and dissociated experience.

THE RELATIONS OF COMPLEX AND SENTIMENT. II.[1]

By A. G. TANSLEY.

To be in a position to discuss with profit the conception of 'the complex' in psychology it is essential to be clear as to what exactly was meant by the term when it was first employed with a definite technical psychological meaning. Rivers's account[2] seems to me misleading, and it thus becomes necessary to go over this ground in some detail.

The general scientific use of the noun *complex* is well known: the term is commonly applied in scientific writing to any group of objects or phenomena which have inter-relations so close that it becomes convenient to regard them collectively as a group apart from other groups, in fact as a secondary unit composed of primary units. Thus the geographer speaks of a 'mountain complex,' a biologist of a 'cell complex' or of a 'tissue complex.' Psychologists have, from time to time, used the term for groups of related phenomena which they wished to consider together, in this perfectly general sense, *i.e.* without implying any *specific* characters as attaching to the 'complex.'

It seems to have been Neisser, using the term in this way, who laid the foundation, perhaps unwittingly, of its modern technical use in psychology and psychopathology—at any rate I have not discovered an earlier use. In discussing the case of paranoids, Neisser writes[3], "the patient is progressively less able to exert an independent control over the course of his presentations (*Vorstellungen*) and thus there arise, in much higher degree than is normally the case, separated groups of presentation-complexes (*Vorstellungskomplexe*) whose contents are only connected through the personal relation which attaches to them all, but which are otherwise scarcely in any way blended with one another.... The affects, which normally regulate our relations to the outer world, and bring about our adaptation to it, which represent a protective mechanism for the organism and the driving forces of self maintenance, are divorced from their natural purposes. Through the organically conditioned strong feeling tone of the illusory (*wahnhafte*) courses of ideas, it happens that on a given mental excitation these and *only these* are always reproduced. This *fixation of affect* destroys the capacity for sympathy and leads to a mental isolation of the patient which runs parallel with his mental aberration."

---

[1] A contribution to the Symposium prepared for the Meeting of the British Psychological Society in Manchester, July, 1922.

[2] In the preceding contribution to this Symposium, "The Relations of Complex and Sentiment" and in his *Instinct and the Unconscious*, Cambridge, 1920, ch. XI, 85–89.

[3] *Individualität und Psychose*, Berlin, 1906, 29.

In the following year Jung, in commenting on Gross's conception of the mechanism of dissociation of consciousness in dementia praecox as depending on the simultaneous occurrence in the mind of functionally separated series of associations, writes[1]: "it seems to me more correct to assume presentation-complexes (*Vorstellungskomplexe*) which become successively conscious and which are constellated by preceding associated complexes. The cement of these complexes is a definite affect." Jung then proceeds to apply this conception to the causation of the symptoms of paranoia, dementia praecox, hysteria and other pathological mental conditions which are due to irruptions into consciousness of presentations and affects from the unconscious, on the lines of Freud's earlier work on the repression of painful memories[2]. He quotes from Neisser the passage, already cited, describing the mental condition of the paranoid, with its fixation of affect, and adds[3], "fixation of affect thus signifies that the repressed (*verdrängte*) complexes (the carriers of the affect) cannot any longer be excluded from the processes of consciousness; they remain and thus hinder the further development of the personality."

It is thus clear that Neisser and Jung introduced the conception of a complex in a specialised sense as a "complex of presentations" (*Vorstellungskomplex*) carrying a specific affect, because they found it useful in elucidating the mechanism of certain pathological mental conditions. But it is equally clear that Jung at least—and it is to him that we owe the establishment of the term in psychopathology—had no thought of confining the *conception* to *repressed* complexes. For instance in contrasting dementia praecox with hysteria he writes[4]: "In dementia praecox ...the new acquirement of complexes is rendered difficult or impossible, the pathogenic complex, or, better, the complex which releases the symptoms, remains the last, and the personality is finally arrested in its further development." The "new acquirement" of complexes by a

---

[1] *Ueber die Psychologie der Dementia Praecox*, Halle, 1907, 30.

[2] It is to be noted that Freud himself did not, at this time, use the term complex in this specialised sense at all, though in current writing the conception is not uncommonly referred to him.

[3] Jung, *l.c.* 39.

[4] *Ibid.* 40.

'developing personality' can certainly not mean the acquirement of 'repressed' or 'dissociated' complexes as such: clearly it must be a process which is either conscious, or of which the results at least have free access to consciousness. In other words the new acquirement of complexes was conceived by Jung as a function of the normal mind, since it is connected by him with 'the development of the personality.'

Jung's further development of the subject makes his idea of the *Vorstellungskomplex* abundantly clear. He conceives of it[1] as a complex toned with feeling (*gefühlsbetonte Komplex*) and composed of "functional psychic units" (which he compares with molecules) each consisting of "components" ('radicals' in the chemical analogy) which are sense impressions, intellectual components, and components of feeling-tone. Each "molecule" or psychic unit brought into consciousness is capable of arousing the complex to which it belongs. The complex is thus a "higher psychic unit," and all its "molecules" share in the feeling-tone of the whole *Vorstellungsmasse*, which is identified with the *gefühlsbetonte Komplex*.

As an example Jung takes the arousing of a particular complex through meeting in the street an old acquaintance, whose image in the mind of the subject is a molecule or psychic unit of the complex, which represents an unpleasant story[2]. The story involves numerous associations, and the corresponding complex consists of many other psychic units besides the image of the acquaintance. The whole mass has a definite feeling-tone in which each psychic unit shares. The bringing of any one of these units into consciousness arouses the feeling-tone common to the whole complex.

Jung then goes on[3] to describe the "ego complex" (*Ichkomplex*) which in the normal mind is the dominant psychic instance, and which corresponds with the *Vorstellungsmasse* of the ego, accompanied by the powerful and always living feeling tone of our own body. Further on[4] Jung takes religion as an example of a powerful complex, and again[5] he has a long description of the state of being in love (*Verliebtheit*) as an example of a complex which for a considerable time may dominate the mind.

I have dealt in some detail with Jung's original treatment of this subject because it seems unfamiliar to several recent writers of high standing, and it is impossible to appreciate correctly the history of a conception in ignorance of the ideas in the mind of its originator at the time of its introduction. Thus McDougall writes[1]: "the word 'complex' was used [by Jung] to denote some idea to which is attached some strong affect, or emotional or instinctive tendency, and which is inaccessible to the voluntary recollection of the subject by reason of repression or dissociation"; and "the pathological complexes which are the complexes in Jung's sense of the word." Rivers writes[2]: "In its original significance, as used by Jung, the term [complex] referred to experience belonging to the unconscious...inaccessible to consciousness...." And even Jones says[3]: "it is much more useful...to keep the word *in the sense given it by Jung*" (my italics), meaning the sense of a repressed or dissociated complex.

---

[1] Jung, *l.c.* 43.

[2] This would probably be an example of a *partially* repressed complex, see below.

[3] Jung, *l.c.* 45.

[4] *Ibid.* 48.

[5] *Ibid.* 53.

The explanation of this prevalent impression that Jung intended the term to apply solely to repressed or dissociated complexes is of course that he, like Neisser, was led to the conception by the consideration of complexes of this nature, and the term immediately became current among psycho-analytical writers in general, who used it for such complexes only, because it was that kind of complex with which they were concerned. If Rivers had been familiar with Jung's exposition of his conception he would hardly have written[4]: "It is an interesting example of Jung's failure to realise the nature of the mental product he was denoting that he should have chosen the term 'complex.' If he had called it 'simplex' he would have been far nearer the truth." When we realise that Jung formed the conception, in accordance with general scientific usage, to represent a united group of units which acquires a secondary individuality, in this case a group of "functional psychic units" connected by common feeling tone to form a "higher psychic unit," and that this conception applies alike to repressed and to unrepressed mental elements, Rivers's criticism is seen to miss the point. Whether repressed complexes are, on the whole, simpler than unrepressed complexes is a distinct question. The point is that both are complexes in the sense intended.

Bernard Hart[5] simply took Jung's conception from its original source, and illustrated it by further examples from the 'normal' mind, such as, for instance, the 'party politics complex' in the mind of the party politician, and the 'photography complex' in the mind of the enthusiastic amateur photographer. Both of these are essentially groups of images and ideas (*Vorstellungen*) which are coloured by common affect, *i.e.* they are 'gefühlsbetonte Vorstellungskomplexe,' and they have thus come to possess, as is very well known, a considerable autonomy in the minds of the party politician and the amateur photographer respectively. Rivers's statement[1] that Hart "greatly extended the meaning of the term" cannot therefore be justified.

---

[1] "A Note on Suggestion," *Journ. of Neurol. and Psychopath.* I, p. 3, 1920.

[2] *Instinct and the Unconscious*, 1920, 84.

[3] *Int. Journ. of Psycho-analysis*, I, 1920, p. 10 of "Book Reviews."

[4] "The Relations of Complex and Sentiment" (this Symposium).

[5] *The Psychology of Insanity*, Cambridge, 1912.

Recently I tried[2], not to depart from Jung's and Hart's conception, but to develop it, by showing that the greater part of the contents of the mind is organised in complexes, which, on account of their constellations of affect and related psychic energy, determine a very large part of our beliefs and actions. I contend that my "system of associated mental elements the stimulation of any one of which tends to call the rest into consciousness through the medium of their common affect" is substantially equivalent to Hart's "emotionally toned system of ideas" which determines conscious behaviour, and that both are intended to apply to the same mental phenomenon which Jung called a "complex of presentations toned with feeling." Rivers writes[3]: "The connotation[4] thus [*i.e.* by me] given to the term is so wide as to destroy any value which the term possesses, as used by Jung and Hart." But I think I have sufficiently shown that the connotation has remained unaltered, and the denotation is extended merely by way of logical development, *i.e.* by showing that the original concept logically applies to a very wide range of normal mental phenomena. Whether this process of development destroys the value of the term is a matter of opinion. Personally I think the value of the term is increased.

The complex is to me a perfectly definite conception applying, as Jung applied it, primarily to a group of presentations which possess a certain degree of autonomy because they are bound together by a specific common affect of a certain degree of intensity, and, it should be added, involve a definite conation or group of conations. Such complexes clearly exist in the normal mind with perfectly free access to consciousness, *e.g.* the 'religion complex' or the 'complex of the beloved person' in an individual mind which are given as examples by Jung. It is, I think, impossible to deny that such complexes, owing to their strong specific affect (or, as I should prefer to say, owing to their specific psychic energy of which the affect is a concomitant and index) have a very considerable degree of autonomy. In fact, as we all know, beliefs and conations of the normal mind, which are in origin independent of the rest of the mind, are determined through such complexes, though they are constantly checked and modified by the action of other mental contents, precisely because there is free access between the complexes in question and the rest of the mental structure. But if now the feeling-tone of the complex becomes painful a certain degree of repression is likely to occur, and *in extreme cases* the complex may become completely unconscious. Since it is no longer in free connexion with the rest of the mind, if it retains its individuality at all, it is said to be 'dissociated,' and its autonomy is enhanced, *i.e.* its independence of action becomes complete, or nearly complete; and if there is failure of resistance to its appearance in consciousness it gives rise to what we call pathological symptoms, or it may even usurp the autonomy of the ego, ultimately becoming practically identified with the ego, as in paranoia or dementia praecox.

---

[1] *Instinct and the Unconscious*, 85, and "The Relations of Complex and Sentiment" (this Symposium).

[2] *The New Psychology*, London, 1920.

[3] "The Relations of the Complex and the Sentiment" (this Symposium).

[4] Presumably a slip for 'denotation.'

But the autonomy of the complex is not, in my view, created solely by repression and dissociation. It has its origin in the separateness within the mental mechanism of the original instinct or group of innate impulses which gave the complex birth because it constellated a particular set of presentations. This separateness is a postulate of modern psychology, and it is illustrated in everyday life by the considerable autonomy of belief and action very generally attained by such complexes as the love complex, the religion complex, the herd complex and many others, which are, notoriously, comparatively little influenced by the co-ordinating rational faculty. It is part of the function of consciousness to keep this autonomy of the complexes within bounds, but this function varies very much in the degree of its success and cannot in any case be *completely* successful in subordinating 'complex action' to central control. In the case of complexes which are somewhat out of harmony with the rest of the mind, there are very varying degrees of repression; and the more a complex is repressed (or segregated), the more it escapes from central control and the more its autonomy is enhanced. Complete repression and dissociation render the autonomy of the complex complete, by withdrawing it altogether from central control.

It is because there is thus a continuous series between the complex which has free access to consciousness and the completely unconscious complex which has no access to consciousness at all that I regard it as important to keep one general term for both sets of phenomena, widely different as they are in their actual relation to the conscious mind and personality. Jung's conception, which was, as I have shown, introduced as a perfectly general psychological conception applying to a normal psychical phenòmenon seems to me to provide a satisfactory basis, and his term seems to me an entirely suitable one.

So much for the development of the conception on Jung's original lines. We have now to face the fact that psycho-analysts, almost without exception, use the term for, and desire its use to be restricted to, what I should call, following Jung, *repressed* complexes. Jung himself in his later writings uses complex almost exclusively for repressed complexes, though I cannot find that he has anywhere repudiated his original conception. The cause of this state of things is clear enough. Psycho-analysts are by profession concerned with the discovery and elucidation of repressed complexes in their patients or their experimental subjects. Those are the only complexes which interest them. Following Freud's essentially empirical method, they rarely concern themselves with general psychology[1]. When framing definitions they include in the connotation of the complex the factor of repression or dissociation[2]. Presumably they hold, if they consider the matter at all, that the term has acquired, since its first introduction by Jung, a more specialised meaning, and that it is desirable to keep it with that meaning. Moreover, psycho-analysts believe they have shown that the really important complexes determining the psychoneuroses are based upon various infantile complexes which go back to what they call the "nuclear complex" (*Kernkomplex*) based on the relation of the child to its parents. Assuming the validity of this belief, it is obvious that such infantile complexes are very different in character from the typical examples Jung quoted when he was describing his conception as a conception of general psychology. They are far less definite cognitively and are already partly or wholly repressed in infancy or early childhood.

Clearly we must leave the psycho-analysts to use their own terms in their own way. Nor is there any great practical confusion involved if the term is kept in its wider meaning in general psychology. Every one who has even a superficial acquaintance with psycho-analytic literature knows what a psycho-analyst means by a complex. Meanwhile it seems to me that Jung's original conception is of very great importance for general psychology, because it represents a universal phenomenon of very great importance in the human mind, and a phenomenon which is common to normal and to morbid minds. The name we give it is of course a matter of secondary importance. It has been suggested that 'constellation' should be employed in the wide sense, but it is certainly not so good a word for the purpose. Personally, in spite of the specialised psycho-analytical use, I see no good reason to depart from Jung's original and very suitable term for the general phenomenon.

---

[1] It may be noted in passing that in more recent years Freud has made several excursions, with very interesting results, into general psychology and what he calls "metapsychology," but he has never, so far as I know, dealt with the general conception of the complex, which is not his term and of which he makes but sparing use.

[2] Cf. for instance Brill, *Psycho-analysis*, New York, 380, and Jones, *Papers on Psycho-analysis*, 2nd ed. London, 1920, 690.

The modern British school of academic psychologists contend[1] that for what I call the unrepressed complex of the normal mind a term already exists which has precisely the same meaning. The 'sentiment' of Shand and McDougall is said to be well established in psychology and to serve the purpose required.

The first difficulty we meet with in examining this claim is that the various authorities do not exactly agree as to what they mean by a sentiment. Thus McDougall explains[2] that his conception, though largely suggested by Shand's writings, involves a distinct and indeed a rival theory, while Myers[3] criticizes McDougall's conception somewhat trenchantly. Into these differences of opinion I do not propose to enter here, but I may be permitted to remark that the attempt to give scientific precision to a word borrowed from ordinary language is bound to carry with it such difficulties, because the popular meaning is vague and shifting and is certain to involve differences in its appreciation by different minds. Indeed, as I have pointed out elsewhere[4], in what is probably its commonest popular use the word 'sentiment' does not refer to single well-defined emotional dispositions such as love or hate, but to what may perhaps be called 'marginal' affect attached to the fringe of a complex. The attempt to analyse and appreciate popular concepts that have been crystallised in language is not of course to be deprecated: on the contrary it is a praiseworthy and indeed indispensable psychological exercise, and will probably lead to considerable clarification of ideas. Only it is doubtful if a workable scientific terminology can be established in this way.

---

[1] McDougall, *An Introduction to Social Psychology*, 14th ed. 1919. Preface, viii and ix; and "A Note on Suggestion," *Journ. Neur. and Psychopath.* I, 1920, 3. Rivers, *Instinct and the Unconscious*, 87, and "The Relations of Complex and Sentiment" (this Symposium): Myers, "The Nature and Development of the Sentiments," *Psyche*, II, 1922, 203.

[2] *Social Psychology*, Preface to 14th ed., vii and viii.

[3] *l.c. passim.*

[4] *The New Psychology*, 5th imp. 1922, 171.

A point on which the authorities appear to be agreed is that love and hate are sentiments, whether we hold with McDougall that these are not "facts" or "modes of experience" but—in Myers' words—merely "psycho-physiological structures," or whether we agree with Myers that they involve the actual experience of the "sentiment feelings." Now love and hate, whether we call them structures, organisations, tendencies or dispositions, are built of emotional stuff—they are *affective* phenomena, whereas, as we have seen, a complex is *primarily* a group of presentations or *cognitive* phenomena, although the complex is a complex in virtue of the common affect attaching to its presentations, and a sentiment is a sentiment because it has a concrete attachment to certain presentations. For these reasons, as it seems to me, we cannot *identify* a complex (unrepressed) with a sentiment without straining altogether unduly the meanings of sentiment, love and hate, as they are understood by the plain man; though we can perhaps say that the sentiment is the consolidated affect of a complex. As I have written elsewhere: "If I love a particular woman and everything connected with her the complex so arising is not my sentiment of love, but the totality of the images and ideas centering round the beloved figure; though this would not exist, as a complex, if the sentiment did not exist."

I agree with Myers[1] that "there seems no reason to make any distinction between the organisation of ideas about an emotion and the organisation of emotions about an idea." In the actual building of a sentiment and of a complex the two processes go on side by side. But the cognitive elements provide the framework, they are the 'carriers of the affect.' And there is a clear advantage in the use of a colourless term like 'complex' for the whole mental structure, rather than a word like 'sentiment' which alike by derivation and in common use applies to emotional stuff—a meaning that we can never divorce from it. McDougall quite definitely desires to include the cognitive elements of the complex as well as the affect in his concept of the sentiment, and if we do that the sentiment of course becomes identical with the complex; but it seems to me more than doubtful if psychology is justified in thus including these elements—alien to the primary meaning of the word—in a conception borrowed from ordinary language.

With much of what Rivers has written about the importance of the process of "fusion" in the formation of the sentiment I quite agree, but it does not touch my contention that we should not use an affective term to include cognitive elements.

---

[1] *l.c.* 203.

To sum up, we have seen that the term 'complex,' as originally introduced by Jung with a technical psychological meaning, signifies a complex of presentations united by a common feeling-tone, and was not restricted to repressed or dissociated complexes, though in the usage of subsequent psycho-analytical writers it has come to have this restricted denotation. In my judgment such a restriction is unfortunate, because it obscures the essential connexion of the normal and pathological phenomena. A word for the whole range of phenomena is wanted and if it is held that 'complex' is no longer available it is possible to use 'constellation,' though little confusion results from the retention of 'complex' in the wide sense, in spite of the specialised psycho-analytic use. In McDougall's use the 'sentiment' is intended to be exactly equivalent to the unrepressed complex, though I think the universal common meaning of sentiment as an affective phenomenon is a grave objection to this identification.

THE RELATIONS OF COMPLEX AND SENTIMENT. III.[1]

By ALEXANDER F. SHAND.

I WILL not continue the historical inquiry as to the original meaning given to the term 'complex' by Jung and his disciples. The question I wish to consider is what now is the best meaning to give to it, and, if we reach agreement on this point, whether we can persuade psychologists in general to adopt it, which does not seem probable at the present time.

The struggle, of course, concerns the extension of the term 'complex' to include certain processes of the *normal* mind; as regards its narrower employment to denote emotional systems that have been 'repressed' or 'suppressed' and 'dissociated' from the normal consciousness, there seems to be considerable agreement. Here there is always some *pathological* symptom to distinguish the 'complex' and the processes of 'repression' and 'dissociation' connected with it. As regards the broader employment of the term, we have to ask whether there is sufficient identity between complexes of the abnormal mind and the so-called complexes of the normal mind to justify our employing the same term for both, or, on the other hand, only a loose analogy between them.

Supposing there is this justification, the question remains whether the term 'complex' is the most suitable for our purpose, or not rather one of the synonymous terms in use in normal psychology before the nature of these abnormal states was so clearly revealed to us by Freud. And here we may consider whether neologisms are always to be preferred in psychology to the readjustment of ordinary terms, from reflecting on the fate which has overtaken the terms 'complex' and 'dissociation.' These during their short history appear to have undergone greater divergencies of meaning than have overtaken the term 'sentiment' during the much longer period of its use as a technical term. Thus we have complexes that are completely dissociated from the normal consciousness, being neither recognised nor controlled by it; others that while remaining in great part out of consciousness, throw up into it some delusion or obsession which the patient is aware of but cannot control; others again that are in consciousness, and have never been repressed or dissociated from it at all; which are under our conscious direction and control, which are wholly normal, and present no morbid features, though they have a "considerable degree of autonomy." But there is as great a difference between an autonomy according to the laws of sanity and that which violates these laws as there is between a firmly established and orderly government and a revolutionary mob.

---

[1] A contribution to the Symposium prepared for the Meeting of the British Psychological Society in Manchester. July, 1922.

We shall now consider the degree of identity between the "considerable autonomy" which is proper and suitable to the purposes of human life and that other autonomy of complexes due to repression, dissociation and the complete loss of control over them.

In the different kinds of volition that occur in the normal mind there are some that are easy because they do not arouse conflict; some that are difficult because they do; and some that totally fail to accomplish the result at which they aim, being succeeded by an action opposed to it, through the compulsion of fear or of some other impulse. Because in this case the action accomplished is opposed to our volition it is called an *in*voluntary action. Yet we can show that from the first type to the last we can introduce intermediate varieties so as to exhibit the whole as a series of gradations that melt into one another, before the extreme or involuntary type is reached at the end. Because of these gradations we should not propose to apply the same term 'voluntary' which denotes the preceding actions to denote also this test or involuntary type. For between the action which is extremely difficult to control and that which we cannot control at all, in which there is an insuperable compulsion upon us, we feel a *hiatus*. So is it with the 'complexes' of the abnormal mind and the emotional 'systems' of the normal mind. The first are completely autonomous because all control over them is lost. The graded autonomy of the second is consistent with control, even where this is difficult. For these reasons I am of opinion that there is no sufficient identity between them to justify our employing the same term for both. Nor can their distinction be satisfactorily expressed by qualifying the first as 'repressed' complexes, or 'dissociated' complexes, because the ambiguity of meaning of these terms is as great as that of the term 'complex' itself. The so-called 'dissociation' of normal and healthy emotional systems strives merely to exclude irrelevant ideas and memories, and to make its process as systematic as possible for the fulfilment of its end. This should not be called 'dissociation.' When a man is concentrated on his work he can quite well recall his occupations at play; but he tries not to do so.

We shall now enter more in detail into the emotional systems of the normal mind, and here our regret at the broad meaning that has been given to the term 'complex' in such an admirable book as *The Psychology of Insanity*, by Dr Hart, is that it serves to confuse the distinction between Sentiments and Emotions. For a complex may be a sentiment of sexual love or party politics, or, on the other hand, some emotion as morbid fear, or some emotional phase of love as jealousy, or an emotion of humiliation or humility, which is detached from the self-regarding sentiment in 'the inferiority complex.'

How is it that the psycho-analytic school, speaking generally, has made no distinction between these two kinds of systems? There are indeed some, in greater or less degree belonging to this school, who have appreciated and emphasized this distinction, as Dr Myers, Prof. McDougall, Dr Brown and perhaps Dr Rivers. But the majority of the school have either not understood it, or not had occasion to draw it. The reason is perhaps that for strictly therapeutical purposes it is of no practical importance. If we lose entire control over a system, it does not matter whether it is what we call an emotion or a sentiment. In either case we have to enable the patient to recover from the evil effects of it and to regain self-control. But from the point of view of pure science our attitude will be different, and a school that claims that its method is one of psycho-analysis should be able to justify its name and not evade distinctions that are only ignored by those who are not psychologists.

And here we have to notice that this new school which has already achieved wonderful results owing to the genius of its founder, and given a 'push' to academic psychology that I should be the last to deny or regret, has yet never analysed the principal emotional forces. Remarkable in the success with which it traces stray words or thoughts or wishes to some hidden source of which the patient is unaware, and in detecting the 'compensations,' 'phantasies' and 'sublimations' of this complex, in other respects it is no more psychological than the attitude of the politician or the journalist; who must refer to human motives, and talk familiarly of fear and anger, love and hate. Hence it is still a matter of some dispute and uncertainty what precisely the master meant by 'the sexual instinct,' to which he attributed such an all-pervasive influence; and these disputes must continue when terms like 'love' and 'hate' are taken from popular thought with its confusion and ambiguity without a precedent analysis.

If we call both emotions and sentiments 'complexes' when they have formed a habitual connexion with some object, or innately respond to the stimulus which it gives them, the term suggests that both are largely aggregates due to chance-associations rather than what they are, namely organized forces in which the ideas and experiences are not merely associated with a common 'affect,' but relevant and subordinate to an end. I, therefore, prefer the term 'system' to the term 'complex,' where we are dealing with the normal mind.

To pass now to the principal question: What importance has this distinction between emotions and sentiments? I will try to show clearly why after more than twenty years' employment of it, I still regard it as of great importance, so that if—as we are liable to be—I am biassed by attachment to my own theories, this may be made evident.

To ask what importance this distinction possesses is something like asking what importance it is for man to possess in the innate constitution of his mind a number of different emotional dispositions instead of a single one. If we can realise what sort of position man would be in if he possessed only a single emotional disposition, we shall be able to realise what would be his position if, while experiencing emotions for various objects, he could never develop a sentiment for any one of them.

What now would be the peculiar disability of man if he possessed only a single emotional disposition?—He could react emotionally to only one kind of situation and not to any other. If the single emotion he was capable of feeling was Anger, he could react emotionally to a situation of conflict when he was attacked or opposed in the pursuit of his ends. But when the situation was different, as one of extreme and pressing danger, he would possess no emotion of fear to impel him to flight, to concealment, or to some other instinctive mode of behaviour organized by this emotion. If again the situation were different and one of disaster to some fellow-creature, he would feel no pity to urge him to relieve it; and similarly of other situations. What man gains by possessing so many different emotional dispositions is therefore the power to adapt himself emotionally to a great many different situations.

We now understand the importance for man of possessing a number of different emotional dispositions, and we pass to the next point. If man lived only a minute, and his situation was constant during this minute, he would at most require the emotion relevant to this situation. But as man is capable of living a great length of time, and during this long time his situation cannot remain constant, if he is to survive, he must be capable of adapting himself to a great many changes of situation.

Owing therefore to the possible length of his life, man requires to be capable of feeling a great many different emotions.

All the different emotions which man through his innate endowment is capable of feeling have come to be organized in the course of evolution for the preservation of himself and of his offspring,—rude and imperfect as is the protection they afford him. This organization, which principally consists of each emotion being related to the situation appropriate to it, man does not make but finds it in himself, just as it is innate also in the higher animals. But man in distinction from most, if not all, animals, forms a sentiment for himself. Whatever else this sentiment is, it includes all these emotional dispositions that bear on his own preservation. Were it only a single emotion it would not avail to adapt him to the changing situations of his life.

Taking these three conceptions of (i) the great duration of some things, (ii) the consequent variety of situations in which they may be placed, and (iii) the need of different emotions for dealing with these situations, they will make clear to us the essential nature of all sentiments. Thus in the relation of man to other beings like himself, we find that to most he is indifferent, to some feels a transient emotion of attraction or repulsion, to a few develops a sentiment substantially the same as that which he forms for himself. As he calls the one sentiment 'self-love,' so he calls the other 'love.' Normally he loves some woman, two or three friends, his family, his country. The personal beings referred to have the same potential duration as himself, are exposed to the same changes of situation, which require them to feel on their own behalf the same variety of emotions. These situations influence those who love them to feel similar emotions. Thus it comes to pass that the emotions of love are so often duplicated, being evoked both in the person himself and in those who love him. These are the 'sympathetic' emotions, aroused sometimes by the expression of the person, sometimes by his situation. A man's sentiment for another is therefore like the sentiment for himself—essentially a capacity of feeling a number of different emotions according to the situation of the other, but distinguished in most cases from self-love, by the presence, not only of the sympathetic, but also of the tender emotions, pre-eminently by gratitude and pity. Hence, he who loves another, in proportion to the degree of his love, takes upon himself the welfare of the loved person, as if it were his own.

It should now be clear to us, not only that a sentiment has great emotional complexity, but that this complexity depends in part on the potential duration of its object. This requires the sentiment to have a capacity for endurance that corresponds with it. We can therefore understand that a sentiment may persist through the greater part of life, and be active for a great part of each day without ever becoming morbid, because this does not involve the undue persistence of any one of its emotions, but an orderly succession of them in response to the changing situations of the object. But this is impossible in the case of an emotion, which if it becomes persistent becomes morbid, as we see clearly in the case of fear. A sentiment is the only system of the mind which is capable, when predominant, of organizing our life as a whole: a predominant emotion disorganizes it.

We shall therefore define the distinction between sentiments and emotions in this way: *the sentiment is a mental system which is capable of adapting itself emotionally to the changing situations of its object, and of persisting indefinitely in correspondence with the duration of its object without becoming morbid: the emotion is a mental system which is capable of adapting itself to only one kind of situation, and when it persists beyond this system tends to become morbid.*

Here we must not overlook the fact that in respect of the one situation to which an emotion is adaptable, there may be considerable variety and change. Thus, for instance, in the one general situation of danger with which fear is connected there may be several particular situations of danger, so that in one the proper response is by flight; in another by concealment; in another by clinging to something else; in another by shamming death; in another by appealing for mercy. Yet, though the emotion has this considerable adaptability, so much greater than any one of its instinctive tendencies, once we are completely out of a situation of danger, it has no legitimate application; and if it persists in the mind we think it ridiculous, exaggerated or morbid.

I hope I have now made it clear why I regard the distinction between sentiments and emotions as of the first importance for psychology. The importance of the sentiment can hardly be denied by anyone who realises what man obtains by his complex and innate emotional constitution; through it the sentiment derives that extraordinary adaptability so much greater than that possessed by any single emotion. For this reason I regard it as of the essence of a sentiment to be constituted of several different emotional dispositions, possessing different conative tendencies. From this point of view we may give a second definition of a sentiment: *a sentiment is a system of several emotional dispositions, having different conative tendencies, connected with a common object, and subordinated to a common end.*

If anyone therefore speaks of "simple sentiments," that is to say of sentiments composed of only one emotion, or calls a phobia a sentiment, to me it seems that he has failed to grasp or to keep clearly in mind the essence of a sentiment, or at least, what comes near to the same thing, that he has defined the word so as to obliterate the point of principal importance.

I have now said enough to make it clear that I am in agreement with Dr Rivers's use of the term 'complex' and in disagreement with Mr Tansley's. Mr Tansley is mistaken, I think, in supposing that the ordinary meanings of 'love' and 'hate' and 'emotion' and 'sentiment' are strained when they are used to include the cognitive as well as the affective aspects. Though the emphasis is laid on the latter aspect, it is no more than this. Love and hate would have no meaning to the ordinary man unless they were taken to include a cognition of their objects. It is very important in psychology to have terms to express concrete facts. Two such terms we possess in the 'Complex' and the 'Sentiment'; and we must resist all attempts to reduce them to abstractions.

THE RELATIONS OF COMPLEX AND SENTIMENT. IV.[1]

BY T. H. PEAR.

ONE of the chief features of this discussion is to be a concerted attempt to attach workable meanings to the words 'complex' and 'sentiment.' The historical origin of the term 'complex' has now been clearly traced, leaving us free to open up other paths. Of those paths the one which tempts me most is that which will lead to an inquiry into the reasons why the meaning of the word 'complex' has 'worked loose'—to borrow Dr F. C. S. Schiller's expressive phrase—so soon. To discuss the factors responsible for this dislocation may help us better to judge whether those meanings which offer themselves as possible supplanters of the original are likely to prove themselves inferior or superior to it.

The object of this paper is to examine some of the meanings—whether they are yet correctly labelled or not—which have come into being as a result of the original expositions by Drs Neisser and Jung of certain conceptions which they had found useful, and of the popularization of these expositions by Dr Bernard Hart and others. I believe that the impetus given to the general study of the subject by these writers has accelerated the crystallization of yet another concept, which may either be termed a complex, or, if that name be denied it, is almost certain to demand one of its own.

I will now endeavour to describe the main features of this concept. Let us begin by accepting provisionally the definition of a sentiment as an organized system of emotional dispositions grouped about an object. This description is so closely packed with meaning that to lay out its contents within the confined space of this article is not an easy task. We will first examine that aspect of it, which in the opinion of some psychologists does violence to the accepted usage of speech; its implication that a sentiment, being a system of emotional dispositions, is never actually experienced, but is part of the structure of the mind. It is true that everyday speech commonly attributes to a sentiment the capacity of being experienced, but our language has probably employed the word in so many ways that more than one meaning might be found by an appeal to it. And my personal experience has not been that the average educated man's sensibilities are very much shocked by this proposal to employ the word 'sentiment' for the enduring organization of mind and body through the action of which he experiences emotions of various qualities and intensities, in relation to an object. Not a few welcome the suggestion that when they love[1] a person, what they actually experience at any moment, when the perception or the idea of the loved one excites in them an affective disturbance, is a blend of emotions, referring to the object; while the fact that they can be said to love someone who is absent not only from their physical environment but from their temporary consciousness, is explicable by the supposition that the person has caused an enduring change in the functional pattern of their mind, and correspondingly, of their nervous system. Love seems to be a force too mighty to be identified with temporary emotions.

---

[1] A contribution to the Symposium prepared for the Meeting of the British Psychological Society in Manchester, July, 1922.

This conception, I take it, is that of Prof. McDougall. Though I am at present uncertain whether the view which will now be outlined is compatible with Mr Shand's own view of the sentiment, I think this to be possible.

Let us now attempt to distinguish sentiment from complex along the lines of the above definition. Both are alike in that they are made up of emotional tendencies. The important aspect in which they appear to differ is this; that while the sentiment is an *organized system of emotional tendencies grouped about* the object, the complex—in the sense of the word which will now be expounded—seems to be a relatively unorganized collocation, sometimes almost a fortuitous concourse, of such tendencies collected about an object.

This difference between sentiment and complex may be discussed from two points of view. We may examine the way in which the internal structure of these two systems differs, or the different kinds of behaviour which they prompt. Let us attempt the first of these tasks.

While the internal arrangement of the emotional dispositions in the sentiment appears to be orderly, those in the complex seem in comparison to be untidily thrown together. One presents the trimness of a Dutch garden; the other exhibits the tangle and the undergrowth of the jungle. It will perhaps be remarked that some apparently tidy gardens contain a corner with undergrowth, usually a little difficult to discover by a casual glance. Moreover, some so-called complexes are very nearly sentiments, but for various reasons are not called by that name; sometimes because it would be unpleasant to recognize them as such, sometimes because society has not acknowledged them or has not yet invented their names. One reason why the sentiments, like the gardens, of a well-bred and fortunate person are apt to be tidy is that they have long been carefully tended by those influential gardeners, his teachers and parents. And it is noteworthy that the two concepts of sentiment and complex, as used in modern psychology, grew up as a result of considering the tidy and the untidy aspects of mind respectively.

---

[1] For obvious reasons this is the sentiment which is naturally chosen as an example when this question is debated.

Let us now illustrate this distinction; incidentally putting it to the test by considering certain examples of behaviour, all of which lie in that vast No Man's Land between orderly and disorderly mentality and defy the psychologist to call them either normal or morbid.

(1) I know a man who in his undergraduate days was devotedly attached to a political party which was working for a certain end, at that time unpopular. He gladly and proudly faced opposition and ridicule, speaking repeatedly in favour of the reform, and was encouraged in person by the leaders of the movement. He went to live in another country where he spread the gospel as energetically as before. One day these leaders, by now famous, came to speak in the town where he was living, and he hastened to greet them after the meeting. Either through weariness or lack of interest they spoke to him perfunctorily and soon bowed themselves away. From that day forward he began to master the writings of persons antagonistic to the proposed reform. In a few years' time he had not only amassed a considerable knowledge of the objections to the reform—but almost imperceptibly, this activity in research had replaced his original propagandist work. All this time, however, he believed himself to be a whole-hearted supporter of the 'cause.' A book on 'complexes' caused him to trace back in his memory the details of this part of his life. He then discovered that his whole change of attitude, his comparative lack of active support, with his theoretical interest in the opponent side, dated back to the hour of his rebuff.

The interpretation of this series of events seems to be that he had not formed a sturdy, 'tidy' sentiment of hatred towards those who had snubbed him, nor that his sentiment for scientific truth had instantaneously spread to this special political subject, but that in place of the original sentiment of affection which was shattered by the rebuff there remained ruins, interpenetrated by new growths; the whole forming an untidy collocation. The result of this upon behaviour was that his conduct towards the leaders, comparatively predictable if a sentiment of love or hate were in question, would, while the complex dominated it, be unpredictable. And it is this predictability of behaviour which is one outstanding feature—though not, I believe, an invariably distinctive one—of the sentiment.

The reader will not unnaturally inquire *by whom* the conduct arising from the sentiment and the complex are predictable and unpredictable respectively. The answer may be given in these words. The conduct of a person possessing a well-developed sentiment or an integration of such sentiments can be predicted by another person who himself either possesses similar sentiments or is the owner of insight and sympathy sufficient to enable him to apprehend the quality of a sentiment which he himself does not possess or possesses only in a small degree. The complex, on the other hand, leads to unpredictable behaviour because of the relative lack of organization and grouping of its emotional tendencies around the object. To the objection that any particular person may judge another's behaviour towards a certain object to be disorderly or unpredictable because he has not appreciated the nature of a sentiment prompting it, I would reply that this is an incidental difficulty of practice well known to the ethnologist and to the psycho-therapist. It points to the necessity of possessing wide sympathies and knowledge if one is to succeed in the task of recognizing a sentiment when one meets it.

(2) Let us now consider the following situations. (*a*) A man is happily married, loves his wife and children and is loved by them. For years he has centred more and more of his pride and ambition upon his family. He now discovers that in order to give his children a good education it will be necessary for him to abandon an expensive hobby.

(*b*) A man is unmarried, rich, and has no relatives or friends whose affection or opinion he values highly. He possesses no very strong moral principles; he has neither religious faith nor sufficient knowledge to lead to agnostic convictions. Up to the present time his general behaviour has been free from serious blame. He falls in love with the wife of another man, and, though he has said nothing of this to her, he suspects that his love is returned.

Most of us, I believe, if circumstances brought us into relation with these two men, would have little hesitation in assuming that in case (*a*) the subsequent conduct is predictable, while in (*b*) it is unpredictable. Moreover, it is probable that if in case (*b*) we find ourselves justified in predicting anything about the man's behaviour, it will be only as a result of our discovery that he possesses strong sentiments for persons or objects connected with this particular situation; for example, the husband or the children.

(3) A single woman is charged before the court with neglecting her children. The magistrates must decide whether in future the children can be justifiably left in her charge. If the justices take their duties seriously they may have to consider whether she possesses any real sentiments for her children, and, if so, whether they are of love or hate, pride or shame, or if her mentality in these respects is so entangled that she cannot be said to have sentiments about them at all, but only complexes. Apart from economic considerations, the solution of this psychological problem is likely to be the chief determinant of their judgment.

I hope that the working value of this conception has now been demonstrated. Whether the collocation of emotional tendencies which we have thus distinguished from the sentiment should be given the name of complex or not, I do not know. But I think that several different conceptions of the complex are reconciled in this one. Yet, the believer in the essentially pathological nature of the complex may ask, is not this 'internal untidiness' the result of repression, and does not repression, therefore, serve admirably as the distinguishing mark of a complex?

I believe that the formation of a complex often necessitates considerable repression, in the wide sense of that word[1], and that its effect is often to render a person's behaviour 'untidy' and unpredictable. But I believe also—and this prevents me from accepting repression as the *differentia* of a complex—that the development of some powerful and important sentiments, at least in society as we know it here and now, implies not a little repression; and moreover, that repression may increase the predictability or the unpredictability of the behaviour arising from a sentiment or a complex, according to circumstances[2]. Although particularly striking examples of complexes are often particularly striking examples of repression, I cannot yet agree that the presence or absence of repression can usefully be accepted as the criterion of a complex.

Alternatively, if the complex be defined in such a way, I think it will encourage psychologists, especially those who deal with the more pleasant and peaceful aspects of mind, to shut their eyes to the repression which often accompanies the formation of a sentiment; to repress, in fact, the thought of repression. Furthermore, there is a risk that ultimately the great value of certain words which now form part of the definition of a sentiment will be discounted. They are the words *organized* and *grouped*; perhaps even *system*. This recognition in current psychological theory of affective organization at a high level should be jealously guarded, for several reasons. One of these is known to every psychotherapist who successfully dissuades a patient from an unwise action by deliberately, and with psychological insight, playing upon his sentiments, and who sometimes fails because around the persons or objects in question the sick man has woven not sentiments but complexes. The other reason is that, at a period of history when the mental differences between man and the lower animals are more difficult to demonstrate than formerly, psychology should lose no chance of bearing witness to one development in which man undoubtedly seems to be superior to them.

---

[1] This would include both Dr Rivers's conceptions of suppression and repression.

[2] Upon the further question whether it can also, according to circumstances, increase the orderliness or disorderliness of organization of the emotional dispositions, I am not prepared to give an opinion.

It is clear that the concept of repression had nearly formulated itself before Freud's time. It was present in psychology, though unnamed, when William James wrote, with that unmistakable stamp of first-handedness, of "some mental objects which for the time being will not develop. They simply *go out*": it reappears repeatedly in his *Principles* and can be found in the *Varieties of Religious Experience*. It is implicit in most modern expositions of attention, memory and thought, and in the physiologist's accounts of inhibition and of reciprocal innervation of antagonistic muscles. Recently, throughout his book, *Instinct and the Unconscious*, Dr Rivers has laid stress not only upon its naturalness but upon its necessity. It would not be surprising, therefore, if it entered into the manufacture of the sentiments, and I believe that it not infrequently does[1].

Let us consider two widespread and important sentiments, love for someone of the other sex, and patriotism. Most civilized persons can appreciate the fact that a particular man should love his wife or his country while, maybe, finding it difficult to understand how it is that the man can love that particular woman or that particular country. Relatively simple, uneducated and aesthetically uncultured minds may form such sentiments without any repression; but can we seriously believe this to be true of a keenly intelligent and cultivated mind? Must not the lover, whether of his country or of his mistress, find some things in her which, in any other land or person, he would wish to be altered, and does not love descend gently upon these defects, clouding their stark angularity?

---

[1] In his contribution to this symposium, Dr Rivers writes that suppression may take "a secondary part in the production of the sentiment."

"Be to her faults a little blind
Be to her virtues very kind"

and, though written in a different key:

"My country, right or wrong,"

do not they describe repression, and is not the behaviour to which they lead all the more predictable because of its action? Again, is it not true of the psychasthenic person that his mind is unusually incapable of forming and of utilizing 'logic-tight compartments'; exceptionally poorly equipped with resistive mental armour which makes life simpler for the un-finicky strong man of action? Probably the 'efficient action upon reality' which occupies the highest level in Janet's hierarchy of actions involves not a little repression if the constitution either of the reality or the agent is at all complicated. If this be so, then to attribute to pathological forces the strength of the apparently healthy-minded and obviously effective 'behaver' would reduce psychological explanation to a string of paradoxes.

It has been urged, in conversation on this matter, as a reason why predictability cannot be taken as the *differentia* between sentiment and complex that under certain circumstances the conduct resulting from a complex may be predictable. If one leaves a claustrophobic patient alone in a strange cellar with the door shut but unlocked, it may be asserted that his behaviour can be successfully predicted. But before we can accept this statement certain considerations must not be forgotten. First, we are dealing here with a type of complex which involves only one emotional disposition, or at most very few. And the collocation of these dispositions does not organize the person's life as a whole, as love and hate tend to do. To borrow Mr Shand's phraseology from the preceding contribution, these dispositions inside the complex are "aggregates due to chance-associations"; not "organized forces in which the ideas and experiences are not merely associated with a common affect but relevant and subordinate to its end." Secondly, the fact that we are considering a phobia and not a fear is of importance. The patient's mind is not squarely filled by the impulse to rush from the room, but this phobic tendency is felt in its characteristic setting of doubt, hesitation and insufficiency, feeble though they may be if the impulse is intense. Given enough incentives he may postpone surrender to his weakness. But what are the most potent of such incentives? Surely, appeals to his well-established sentiments; to his regard for the good opinion of others and of himself; his courage or his love. Any of these, if they be strong, may compete successfully with the phobia.

But these incentives also have their behaviour counterparts in muscular and glandular changes, such as inhibition of movement or alteration in quality or amount of secretion. And since the 'incompleteness' of a phobia is one of its main characteristics, it may be concluded that the *complete* behaviour resulting from a complex is by no means predictable.

At this point, then, it may appear that after all, repression is the cause of the internal disorderliness in a collection of emotional dispositions relating to some object, and that the love or patriotism which have developed partly as a result of repression *must* be complexes, however reluctant people may be to acknowledge this state of affairs. This might be one way out of the difficulty, and it would have the merit of recognizing adequately the rôle of repression in sentiment-making. But there appear to be at least two cases which make this conception appear too simple. Mental growths possessing this characteristic of internal untidiness, and therefore unworthy of the name of sentiment, arise during the early stages of the formation of a sentiment, and during its decay or death. The 'love' for its parents of a child under six months of age can scarcely be termed a sentiment if that term is to be given the definition which we have accepted. For within the same five minutes such a child may display towards its parents the most startlingly varied emotions, some suggesting the presence of love, others of hate if we were to interpret them as signs of actual sentiments. But the inconstancy of such a young child's behaviour is its most striking feature, contrasting vividly with the regularity of its conduct when, a few years later, it has had time and opportunity to form real sentiments which come to its support in certain situations. At the other end of life, decay and disintegration may bring about in the sentiment a somewhat similar untidiness of internal organization when it once again becomes a complex.

Lastly, a complex, or a set of them, may result from the long-continued conflict of two sentiments, each of them strong and internally well-organized, but leading to incompatible lines of conduct. In a similar way two well-disciplined armies may fight until both are demoralized and lose their integrity.

Such a conception, I believe, reconciles several points of view. The oft-quoted examples of golf and photography appear as instances of sentiments in some persons; complexes in others. It seems to accord with medical, psychological and legal views of aberrations of behaviour.

Lastly, it lends itself to interpretation along the lines of 'behaviourism' and of physiology. Theoretically, the emotional dispositions or tendencies in the definition must be capable of translation into terms of their physiological concomitants. The functional inter-relation of their neural controlling mechanisms may be complex or simple, 'tidy' or 'untidy.' The dominant pattern of the sentiment may be like a well-arranged switch-board, that of the complex like an untidy bunch of live wires. From the latter we may expect many more short-circuits and perhaps, breakdown of insulation; but this is a line of thought which cannot be pursued in this paper.

In this view, psycho-analysis would be a sorting out and disentangling of the emotional tendencies in the complex, a 'straightening,' to borrow Samuel Butler's term. Re-education, whether deliberately carried out or arising incidentally during the treatment, would be the attachment of the liberated energy to other worthier objects, and the formation of more effective sentiments about the original objects. And if this be correct we can understand how mere analysis, without re-education, may leave a patient with no ethical or other values potent enough to lead him successfully through life.

In conclusion, perhaps I may be pardoned if I introduce an analogy which may make some of these points clearer to the untechnical reader. If we consider the fact that a single object in the environment does not usually produce in the average man one single pure emotion, but rather a blend of two or more emotions, the likeness of his experience to the chord in music is naturally suggested. May we compare the sentiment to an organization of potentialities for producing blends of musical notes?

Such an organization exists in the moving roll of paper which actuates the notes of an automatic piano player. Whenever a hole in the paper passes a corresponding hole in a brass cylinder, a note is played. The pitch of this note depends upon its horizontal position on the moving roll; its intensity upon the size and its duration upon the length of the hole. Blends of notes occur when holes in the same horizontal line pass across the cylinder.

We may compare the roll to the material substratum, the physiological basis of the sentiment. The notes heard may be likened to the emotions which are actually experienced. Just as on Prof. McDougall's theory, the term sentiment stands for something which itself is never experienced but is part of the structure of the mind[1], so the roll, though it is never heard, is the material bearer of the organization of the notes. And if the reader objects—as he ought to—that the roll is dead while the physiological basis of the sentiment is living, let us suppose that, a few years hence, musicians will record their compositions directly on the paper roll and that it will therefore be usual for a composer to alter the pattern of holes every day during the growth of his composition; widening and narrowing some holes, lengthening and shortening others, and occasionally adding new or obliterating old ones.

---

[1] Mr Shand's definition of a sentiment includes the phrase "a system of several emotional dispositions...."

We know that, mathematically regarded, the number of possible combinations of notes which can be played simultaneously on such an instrument is infinite: we know, too, that the number which is actually played in practice, though huge, is much smaller than infinity and that the laws of fusion and consonance explain why this is so. So it is probably with the sentiment, for the emotions may have their own laws of fusion and consonance. We know again that a person accustomed only to the simpler musical intervals and chords may declare that our modern music is but a collection of noises; we know too that its composers can often demonstrate quite convincingly that their use of certain 'new' intervals is but an extension of laws already known. Does not a similar situation exist in regard to some of our modern sentiments? Do not old-fashioned people complain bitterly and sometimes justly, of the untidiness of sentiments in the twentieth century as compared with the tidiness of those in the nineteenth? May not the explanation be that they, in the famous phrase, have a bad ear for new music?

And so our analogy brings us again to the question of predictability; by and for whom? The answer is simpler in the realm of music. Though to the listener who is only accustomed to the simpler chords and sequences the modern compositions are unpredictable, the student who has analysed them finds that much of their mystery vanishes. So it may be with the psycho-therapist or the priest who himself brought up in a tidier world is trying to understand minds which have developed in the tangle of the last ten years. He often finds it difficult to decide whether the emotional dispositions of such personalities are organized in sentiments or complexes—and no wonder. Yet he can learn something from the attitude of musical critics throughout the ages.

And this analogy is only strengthened by the fact that in spite of this tolerant attitude towards new music everyone recognises that there do exist cacophonies which cannot possibly be fitted into any known laws of consonance. This complete lack of organization of emotional tendencies would characterize the extreme instance of the complex.

It may be not without interest to continue this analogy a little further. On hearing a few bars played by an instrument such as that which has been described, an expert, judging merely by the particular blends and successions of notes may often identify the music as definitely characteristic of a certain composer, a certain school, or a certain period. So we can sometimes confidently speak of certain sentiments and integrations of sentiments as characteristic of certain persons, perhaps indeed of certain nationalities at certain times. Along the lines of such an analogy, the integration of sentiments is not difficult to conceive; nor is the disintegration of a sentiment into a complex as a result of the continued introduction of intense emotions of a quality foreign to those already represented in it. And lastly, if we suppose the holes in the roll to be blocked up, added to or enlarged through the action of damp or mould, we can imagine the process by which, under the influence of bodily disease, a sentiment may decay.

THE RELATIONS OF COMPLEX AND SENTIMENT. V.[1]

BY BERNARD HART.

THE conceptions of complex and sentiment have developed as the result of investigations conducted along different lines of approach, and in this historical fact is to be found the explanation of the overlapping of their denotation, and of the difficulty which is now being experienced in finding satisfactory lines of demarcation between them. The concept of the complex has been evolved by writers whose main interest lay in the pathological or, as Pear aptly terms it, the "untidy" aspects of mind, whereas sentiment has been the product of researches directed almost entirely into the normal or "tidy" mind. Nevertheless, as Tansley has clearly shown, complex was not originally used by Jung as having a necessarily pathological significance, although it can be easily understood how the direction of his interests and of those of the psychoanalytical school in general have led to the ultimate limitation of the term to morbid conditions. In his *Psychologie der Dementia Praecox* Jung's chief aim was to discover the dynamic factors responsible for the phenomena in that disorder, and he found these to consist in collocations of mental elements carrying specific affects to which he gave the name 'complexes.' He indicated, moreover, that dynamic factors of a closely similar kind could be found in action in the normal mind. Clearly he was here approaching a conception identical in many respects with the 'sentiments' which McDougall reached as the result of a different line of approach. It is important to bear in mind in this connection that Jung's book was published in 1907, that McDougall's *Introduction to Social Psychology* did not appear until 1908, and that Jung was presumably not acquainted with Shand's earlier work.

My own line of development was by way of Jung and, although I was acquainted with McDougall's work, I had not clearly formulated in my mind its relations to Jung's conceptions when I wrote my *Psychology of Insanity* in 1911. It is necessary to emphasize this latter date, because eleven years is a long time in the history of recent psychology and, were I to re-write the book now I can unhesitatingly say that I should not use the term complex to indicate the conception developed therein. Nevertheless I still believe the conception itself to have a certain value, and I shall endeavour to set out here the grounds upon which that value rests.

---

[1] A contribution to the Symposium prepared for the Meeting of the British Psychological Society in Manchester, July, 1922.

If we seek to discover the causes determining the direction of flow of our thought and conduct we find amongst these causes systems of mental elements to which, following Jung, I gave the name of 'complexes.' The essential feature of each of these systems is that the constituent mental elements are linked together to form a 'higher psychic unit,' which has a more or less definite conative trend, and which therefore tends to influence the flow of thought and conduct in a definite direction. It is in virtue of this conative trend that the 'complex' can be regarded as a unit, and I desire to emphasize that this and not the possession of a common affect is the central character of the concept in question[1].

If a complex is subjected to further analysis it is apparent that the conative trend is due to the incorporation within it of one or more instinctive processes, using this latter term in the sense developed by McDougall. The answer to our inquiry concerning the causes determining the flow of thought and conduct must therefore ultimately be formulated in terms of these instinctive processes, but at a more superficial level it is possible to distinguish agglomerations in which instinctive processes and other mental elements are linked together, and which act as functional units. These functional units have always this much as their common feature, that they act as determinants of thought and conduct, and that they are composed of mental elements linked together and incorporating one or more instinctive processes, and this degree of similarity would seem to justify the attribution of a common name to all functional units possessing these characters. Rivers[2] admits that both complexes (in Rivers' sense) and sentiments determine thought and conduct, but holds that they differ profoundly in other respects and that therefore nothing but confusion can result from including them in one category. But surely, if they both determine thought and conduct it is profitable to inquire whether common features cannot be discerned upon which this common property depends, and if such common features can be formulated then we have a concept which is of value in helping us to understand the dynamic aspects of mind.

---

[1] If, of course, we regard 'affect' as the central limb of an instinctive process, the 'common affect' would have the same practical significance as 'conative trend,' but to define complex in terms of affect tends to obscure the dynamic aspect which is its essential feature.

[2] Rivers, *Instinct and the Unconscious*, 2nd ed. Cambridge, 1922, 88.

It is of course beyond dispute that the concept so formed has a very wide denotation, and that it includes a number of processes which differ from one another in important respects. In other words there is clearly room and necessity for a subdivision of complex into subordinate classes, for example into repressed and unrepressed, or into sentiment and not-sentiment. But this subdivision should not obscure the common dynamic character which runs through them all. The complex which is repressed exerts its effect on thought and conduct in virtue of the same driving forces which act in the case of the complex which is unrepressed. The circumstance that it is repressed modifies the action of these forces, but the repression is merely a secondary and superadded factor. The Freudian 'nuclear complex' produces its effects because of the component of the sex instinct which forms its core, although the action is distorted by the repression to which it is subjected. If it were not repressed its effects would be different, but it would not produce something of an absolutely unrelated character.

The objection that it is undesirable to include under one category factors of a normal and of a pathological kind is of questionable validity. The attempt to subdivide relatively simple mental processes along these lines is, indeed, in my opinion fundamentally vicious, in that it tends to obscure the essential identity of the factors concerned. 'Normal' and 'pathological' have meaning only when we are considering the relation of the total mental economy to the welfare of the individual. Repression in itself is neither normal nor pathological, and it may lead to results which are either desirable or noxious according to the total setting of the picture of which it forms a part. Pear has developed this point in his contribution to this Symposium, and he remarks that Rivers in his *Instinct and the Unconscious* has himself insisted throughout upon the naturalness and necessity of repression.

Nevertheless, it is clear that repression is an important superadded factor in determining the effect produced by a complex, and we may accept as a first subdivision of the concept that into repressed and unrepressed varieties. Whether or not we decide to limit the name complex to the former variety only is purely a matter of terminology, but if it is so limited then there is room for another term to indicate the wider concept.

Sentiment, according to McDougall's definition, would seem to be practically identical with complex, and McDougall[1] recognises this explicitly in his *Social Psychology*, although he thinks it desirable to limit the term complex to "morbid or pathological sentiments." There seems, indeed, to be no reason why a sentiment, defined as McDougall defines it, should not be repressed, and a system of emotional tendencies organised around some object may continue to act as a functional unit, even though its action is modified and distorted by repression. Evidently, however, this definition is not sufficient to delimit sentiment as it is understood by other contributors to this Symposium, and to describe all those functional units to which the name of complex may be applied as sentiments would certainly strain the meaning of the word far beyond that which it has in common speech. The further limitation which is desired beyond that covered by the words of McDougall's definition seems to depend essentially upon the degree of organization attained. This factor has been dwelt upon at length by Pear, and he proposes to separate off from the relatively perfectly organized system of emotional tendencies to which he would limit the name of sentiment, another type of system consisting in "a relatively unorganized collocation, sometimes almost a fortuitous concourse, of such tendencies."

Organization might be taken to refer either to the internal organization of the complex or to the organization of the complex with the mind as a whole. In the former case sentiment and not-sentiment would constitute a line of cleavage which has nothing to do with repression and not-repression. If, however, organization refers to organization with the whole structure of the mind, then the line of cleavage between sentiment and not-sentiment would bear an indirect relation to that between repression and not-repression, although it would be by no means identical therewith. This indirect relation would arise from the circumstance that repression would necessarily limit the degree of organization of the complex with the whole structure of the mind, and repressed complexes therefore would be unlikely to attain to the dignity of sentiments. Predictability of the effect of a complex or sentiment is partly dependent upon the same factors in that it tends to be greater in the case of a unit whose functional integration with the whole structure of the mind is more extensive and highly organized, but it is also dependent upon other factors such as the internal organization of the unit and the potency of the instinctive forces incorporated therein. Owing to the latter the degree of predictability may be high in the case of complexes of relatively simple structure.

---

[1] McDougall, *An Introduction to Social Psychology*, 16th ed. London, viii–ix.

It is apparent that there is no unequivocal consensus of opinion as to the exact meaning to be attached to 'sentiment.' McDougall explicitly states that he is not in agreement with Shand on this point, and McDougall's definition certainly does not necessarily imply the relative perfection of organization which Pear requires before a functional unit can be admitted to the rank of sentiment. If, as would seem desirable, sentiment is only applied to units which satisfy this criterion, then the relation of sentiment to complex, in the sense in which I have used the latter term, would be that of a subordinate to a wider concept.

Rivers' criticism that a product of suppression should not be classed with a product of fusion approaches the subject from an angle different from that which has been attempted above, in that the developmental rather than the dynamic aspect is accentuated. Rivers has pointed out an interesting biological analogy between certain mental and physiological processes, which casts light upon the way in which the mechanism of repression may have developed. But from the dynamic point of view the essential determinant of thought and conduct is to be found in the system with its conative tendencies, and whether this system acts by the method of fusion or that of suppression is a secondary and superadded character.

We may summarize the considerations set out above as follows. The term complex, as employed in my *Psychology of Insanity*, is attached to a concept which is of value in that it marks off a class of functional units, all of which have certain common features. Its denotation is, however, very wide, and it is essential that the concept should be subdivided into subordinate classes. The most obvious of the possible subdivisions is that into repressed and unrepressed. The use of the term complex has tended to become limited to the first variety only, but this is in my opinion unfortunate in that it tends to obscure the essential similarity of both varieties as functional units. Sentiment and not-sentiment are dependent upon a line of cleavage which is only indirectly related to that between repression and not-repression, and they involve in common speech, and in the usage desired by several recent writers, the taking into consideration of the degree of organization which the functional unit possesses. Sentiment would therefore be limited to a comparatively small class of functional units distinguished by a high degree of integration and organization with the whole structure of the mind.

THE RELATIONS OF COMPLEX AND SENTIMENT. VI[1].

BY CHARLES S. MYERS.

IT is obvious that agreement on this subject is impossible until acceptable definitions have been framed of the two terms which are to be related In order to reach greater precision, psychologists have attempted to change the meaning of these and other words in common use, but without paying enough attention to their current meaning or to the route by which they have come to acquire that meaning. It is too generally assumed that popular usage annihilates meaning. This is an error: it is always possible to define the words of common parlance, and it is of considerable psychological interest to study their significance.

Thus there is good reason why we popularly speak of 'feeling' anger, cold, jealousy, pressure, etc., whereas we do not speak of 'feeling' sounds or colours. There is also good reason for declining to regard 'sentiment' as a mere psycho-physiological structure or disposition—unless we reserve the term 'sentiment feeling' for the specific feeling which may accompany it. As I have said elsewhere, "there are two distinct ways in which the term 'sentiment' may be used, the one involving the actual experience, the 'sentiment feeling,' of love or hate when the object of the sentiment is seen or thought of, the other being the sense in which one may, in the presence or absence of that object, be said to love or to hate him without experiencing the sentiment feeling of love or hate. So it is with feelings of quite other kinds which have become strongly associated with any given object. Thus I may state that I desire something, that I am bashful or hopeful of doing something, or that I am envious of or sympathetic towards or even angry with someone—without necessarily experiencing at that moment the feelings of desire, bashfulness, hope, envy, sympathy or anger"[2]. Sentiment hence covers sentiment disposition and sentiment feeling. Sentiment feeling is more stable than emotional feeling: sentiment is not merely organized by but also organizes the various emotions which can assist its end: it is dependent not only on acquired experience but on inherited dispositions peculiar to the different sentiments. An emotion is related rather to a broad situation, a sentiment to a definite object or idea on which it confers a certain 'value.' In the former a single feeling, in the latter a variety of feelings, is evoked.

---

[1] A contribution to the Symposium prepared for the Meeting of the British Psychological Society in Manchester, July, 1922.

[2] *Psyche,* 1922, ii. 196–204.

The popular notion of a 'complex' is, I think, founded on surprise, irrationality, and inexplicability. We ascribe a complex to our neighbour when he behaves in a surprising, irrational manner; we attribute our own conduct to a complex when it appears to us inexplicable. But not every surprising, irrational, inexplicable act is necessarily ascribable to a complex. The act must arise from a surprising, inexplicable impulse (which, as in the psycho-galvanic response, need not be actually experienced); that impulse must arise from a surprising, inexplicable affect (which again need not, in its original form at least, be experienced); that affect must in turn arise from a 'forgotten' situation,—a cognitive experience which has become dissociated, repressed, suppressed from or inhibited by (whatever be our preferred terminology) the normal personality.

We conclude then, that, on the one hand, psychologists have not gone far enough when they define sentiment as a system of various emotional dispositions fused about a common object, and that, on the other hand, they have gone too far when they define a complex as a series of cognitive experiences united by a common affect.

If we modify these two definitions in the way I suggest, carefully distinguishing sentiment disposition from sentiment feeling, and employing complex in its narrower sense, we find ourselves bound to conclude that sometimes a sentiment feeling may grow from a complex and assume certain of its characters. Clearly hate is a sentiment feeling which we may at times entertain, towards an individual, sex, or abstract idea, without in the least understanding *why* we hate; we may even discover on analysis that we have a repressed, unconscious sentiment feeling of love or hate towards some object or concept. Moreover, owing to the complex on which it is based, a sentiment feeling may be subject to inversion, projection, transference, etc.; and it may show characters very different from those of gradation, 'tidiness,' and 'predictability' which previous contributors to this symposium have variously held to distinguish the sentiment from the complex. In such circumstances the sentiment may justly be regarded as a complex.

Some contributors have supposed that the sentiment, in its 'tidy,' 'predictable' form, is necessarily the product of a *normal* mind. But we cannot differentiate complex from sentiment on the basis of morbidity. In the psychoses (the 'true' insanities), *e.g.* in paranoia, the sentiment feelings of love and hate, and all their modifications, are fairly 'tidy,' 'organized' and 'predictable'; could we but enter intimately enough into the deranged mentality of the person experiencing them, we should be able to trace them clearly and logically to his delusions. Again, complexes are not necessarily 'morbid.' Our daily prejudices are often due to them; surely these are not to be termed 'pathological.' Lastly, just as our sentiment feelings may not always be finely graded, so our complexes need not always be of the 'protopathic,' 'all-or-none' type. We must not confound loss of higher control with complete absence of gradation; the two are by no means synonymous. All that we can safely say is that with higher control comes greater delicacy of gradation.

I conclude, then, that a complex is not always morbid, that a sentiment is not always healthy, and that the sentiment may become a complex when the latter enters into the constitution of the former.

A NOTE ON SOME DREAMS OF A NORMAL PERSON.

BY MARY STURT.

THE following series of dreams which has come into my hands seems of interest as belonging to a person struggling with a conflict of which she was fully conscious. When it is so frequently assumed that the conflicts which are represented in dreams belong to the 'unconscious,' evidence that this need not be so is of value. Further, these dreams lend some support to the theory that poetry and dreams can take their rise from the same emotional disturbance. For with the dreams certain verses were also sent me. These verses refer to the same events as the dreams and the method in which they were written is described as follows: "I am conscious of a desire to write poetry, but there are no words in my mind. Presently I find myself saying over the first line of a poem. If I take up a pen the whole of the rest of the poem follows at once without the least effort on my part, and if a line is incomplete or omitted I find it almost impossible to put it in afterwards by conscious effort." This description seems to make the process of composition unwitting and to remove it from the direct control of consciousness, although, of course, the actual writing is carried out in a conscious state. It may be added that my correspondent records that she dreams poems fairly frequently, though they are always forgotten on waking.

As to the actual records, the dreams, with the exception of the first two, were written down at once on waking and were dated. The first two were not written down until a short time after their occurrence; but they were told, and the analysis was thought of, at once. The poems were also dated and have not been revised in any way. Where an analysis is given with the dreams, it is the dreamer's own and was written down either at once, with the dream, or in the course of the following morning[1]. I have not attempted to carry these analyses any further, partly from want of skill, partly because their general lines seem so clear that they need no further explanation. The lack of disguise in most of the dreams may be due to the fact that the dreamer was at the time consciously considering the problem concerned in the dream. It will be seen that there is least disguise in the dreams in the middle of the series when the *conscious* disturbance was at its height.

---

[1] The dreamer had some knowledge of modern psychology and was acquainted with the various methods of dream analysis.

The dreamer, whom I will call *X*, was 24 and had recently gone down from college. For two or three years past she had been perplexed on the subject of 'ardent' friendships between women. Although she had always had intimate friends, her friendships had been of a quite unemotional kind; but during her years at college the problem presented itself in an unpleasant manner, since here she formed a friendship with *K* of which she came to disapprove. It occupied too much of her time, and led her to neglect friends whom she really valued more highly. It also exhausted her emotionally, as *K* was her intellectual superior and was of a definitely neurotic character, occasionally showing strong suicidal tendencies. At the same time she felt completely unable to end the friendship until she was parted from *K* for a long period.

Owing to certain other circumstances this problem was still in her mind when she met *Y* at college about a year and a half later. They rapidly became friends, *X* valuing the friendship highly, partly for the pleasure she derived from *Y*'s company, partly for the intellectual stimulus she found in her. At the same time *Y* was demonstrative in her manner, and *X* dreaded becoming too fond of her and too dependent on her for ideas. So long as *X* and *Y* were still at college, this friendship brought mainly pleasure to *X*. The following verses, written by her after a long week-end in the country, illustrate this satisfaction:

A. June 22. There is a road, and we have walked on it,
1920. A hill-top road and round us lay the plain,
There is a road and we have talked on it,
Of men and life—the laughter and the pain.

There is a tower and we have sat by it,
And read us books, and watched cloud shadows pass,
We had a charm—we will not tell the name of it—
That lent a splendour to the very grass.

There was a time of wind and sun and poesy,
To be a strength for working, and a toy for memory.

Shortly after this they were separated, *Y* taking up professional work. About August *X* was staying with *Y* and the following dream occurred:

I. "Dreamed just before waking in the morning. The room was full of light.

I was on a steep knoll in a wood with *Y*. She was trying to address some persons and I was driving away certain animals that kept jumping up at us. A big dog did it several times, and at last I sat down and bent forward and caught it with both hands round the head. I called to someone below, 'Come and cut its throat.' *Y* said, 'That is cruel. Why not just drive it away?' I was determined, and held on waiting for someone to do it. *Y* sat down behind me (I was lying almost flat now, with my arms over the edge of the knoll holding the animal) and she put her hands on my back with the thumbs against one of the lower ribs. (The position was nearly the same as for artificial respiration.) She pressed hard with them causing me great pain. I would not let go of the dog, however, but I could hardly bear the pain; so I said irritably, 'Are you never going to do it?' Then a man below said, 'It is done.' I looked and saw that the dog was dead with his throat cut. I expected *Y* to leave off her pressure then, but she continued it. I was too proud to say anything, but I was no longer sure if I could control my face; so I dropped my head, laying it on the curve of my arm.

I woke up there, and at once experimented to see if I could produce the same pain. Though I tried hard I could produce nothing like such an acute one by the same means."

The analysis as far as given was thought of before breakfast. "A few days before, I had been lent Masefield's book, *Enslaved*. I had been much struck by the sonnet series 'Animula,'—in particular by one of the end ones that describes the death of the 'scholar'—and contains the line, 'And soul the stag escaped the hound desire.' As I had not been able to send *Y* the book, I had copied out the whole series and sent them to her. I had also read them to her on my arrival, and discussed them, but mainly from the side of construction. The dream was certainly due to the line I have quoted. I feel little doubt that *Y* would have resisted an attempt to break up the friendship. I believe the pain (it had no discoverable physical cause) was indicative of the pain I should certainly feel if the friendship were broken. The man below who was asked to cut the dog's throat may have been due to the fact that I had been thinking that *Y* was almost certain to get married soon to some one, and then anyhow we should be divided."

The friends met again during the vacation, but for the next year they worked in different towns. This caused *X* many gloomy thoughts. She was also troubled by the amount of time she spent in thinking of *Y*. The following poem was written after a meeting when they had read Shakespeare's *Sonnets* together:

B. September 22. Oh! I should love you only for your mind,
For knowledge and sweet converse and bright wit,
The books we read, the passing of the wind,
The sunshine and the garden where we sit—
What more should I desire than that we two
Should learn together, and our knowledge share,
Divide one thought, see one clear sky of blue,
Watch evening gather on the city there—
I should not lie o' nights and long for pleasure
To touch your hand, to kiss your lips and eyes,
Clasp arms about you, feel your body's measure
Cling, hold you fast, and crush you as my prize.
It is not right to love your beauty so,
And yet I love it where so e'er I go.

For the next two months they did not meet. Dream II occurred in November.

II. "I was in a room on a bed. *R* and *S* (two male colleagues of *Y*'s whom I had never seen but had heard about) were in the room defending the door. We were in a building which was being besieged by a mob of Bolsheviks. In the room were also two dogs, one small and ugly, of which I took no notice, the other a big one. The latter came to me and I feared he was going to bite me, but I put out my hand and he took it in his mouth, but did not close his teeth on it or do it any harm....

Then I was outside the building with *Y* and her brother. I was asking them not to enter as the crowd might be dangerous; they would come, however. We went in. It was a grand-ducal palace, and we walked through the rooms, admiring the things in it. I said, 'What a pity all this must be destroyed!' Later we had a meal together of cutlets."

The analysis was thought of at once on waking. "The dog was certainly the same as before, and represented my surrender to the friendship. *R* and *S* probably referred to my thoughts of *Y*'s possible marriage. Entering the house referred to the life I should like to share with *Y*."

This dream was sent to *Y* but without the analysis. In an answering letter *Y* sent the following analysis of the dream, which agrees closely with *X*'s own, though it is more detailed. (*Y* knew of dream I and the analysis of it.)

The letter runs: "Let me play the interpreter. Thy dream, O great one, is thy desire to admit and give back affection. The dangers of its presence are great, and you did right to suspect them. At first it seemed almost good to kill the dog of this particular breed. There are other dogs that one takes for granted, or whose presence seems natural. But you let the dog live, and perhaps since you became friends with it, it has shown some qualities pleasant to you. And so despite the fact that it seemed revolutionary to you—yet withal a revolution with which you more than half sympathised (for such I take it is your attitude to Bolshevism)—you entered into the house of friendship with certain people, although you implored them not to enter it; and having entered again, you found and acknowledged that they also have found certain delightful places within the house, and it must all decay and tragedy walks upon the scene. They all three know that the house must fall (although not by their own acts, but by a blind mob-attack as blind as Fate) and they turned and ate cutlets."

There are no other clearly relevant dreams recorded until the following spring. The relations between the friends remained unchanged, although *X* was gradually becoming convinced that *Y* would shortly be engaged to one of her colleagues. *X* and *Y* met at Christmas, after having been apart during the preceding term. *X* was rather lonely and very tired of life in lodgings.

III. May 6. "*Y* and I had arrived at a school. It was a convent and standing in an orchard with apple trees in bloom. On arrival we went into a cloak-room. We were very hot. *Y* went off to wash....I found her stripped to the waist to wash. I went up to her and threw my hands round her and kissed her. She was very cool and my hands on her back felt the skin cool and smooth."

There is no interpretation given of this dream, but the meaning is sufficiently obvious. Of much the same import though with a different solution of the conflict suggested is a rather vague dream given under the date May 14.

IV. "In a room together going through the ceremony of signing a book. The process equivalent to a partial marriage, but ceremony not complete. It was an ordinary sort of lodging-house room. We separated.... I was out on a cycle and went and collected from another town certain letters and telegrams for *Y*. I then went back to the first house and completed the ceremony alone. I then went home. I went up to the nursery. There I found my brother. I said 'We are married now,' and put my hands on his shoulders. He began to talk excitedly. I remember his voice shouting. I begged him to 'shut up'; it was clearly a secret."

The note added is "The other person was really *Y*." The identity seems to have been left vague in this dream itself.

About the same time *X* heard that *Y* had received a proposal of marriage from *S* and would probably accept it. This seemed to *X* to threaten a speedy break in their friendship and it grieved her considerably. After a week or two of worrying, the following dream occurred:

V. July 1. "Dreamed in the morning just before waking. I had been crying the night before in bed. The dream began confusedly—something about a street-singer and an instrument—possibly a banjo—that I wanted but could not have. I was up in an upstairs room with *Y* and her mother. There was a window which reached nearly to the floor and cushions on the floor. There was also a bed in the far corner. Mrs *Y* kept in the background. I was lying face downwards on the cushions. *Y* was kneeling beside me. Something had been said about my wanting a banjo, and she asked 'Did *K* play one?' I said 'Yes' and cried. I understood the remark as a reproach and was bitterly ashamed. I struggled and said 'let me turn over.' She did...."

The comments run: "*J*, *Y*'s brother, played the banjo. *Y* and her mother had been staying in the town and I had wanted to talk to *Y* alone but had found it difficult. The point of the dream seemed to lie in *Y*'s question. It filled me in the dream with an overwhelming sense of shame and contrition." [This is added later.] "I felt all the morning as if I had been severely scolded, and I could not summon up courage to tell the dream to *Y* for some months. I believe the question was understood to refer to points of similarity between my friendship with *K* and with *Y*. I had always objected to this idea. Also to the fact that I had consoled myself for *K* and might do so again. Anyhow, I took the dream to mean that I was making a fool of myself by my present behaviour and had better cheer up."

X's efforts were fairly successful and the party spent a very pleasant holiday together. In the latter part of the long vacation when *X* and *Y* were again separated, there was a relapse.

C. September 2. Lesbia, I love you for your beauty's sake,
For your sweet mirth and maddening joyousness,
And for your moods of sadness. But I hate
In that you are not mine and mine possessed.
Not having all, I still have ever less
Than having nothing. I am grudging greed,
And hungry-eyed desire. I'd rather starve
Than eat my half of the feast. And I'm a fool
And all my folly know and weep at it,
And turn and twist and rack my mind with thoughts
Till torture self-inflicted tires me dumb.

This note is added: "The last line is a reminiscence from one of the lines in 'The Lemming' from *Enslaved.* It puzzled me for two or three days till I thought of it."

At this point *X* discovered Baudouin's *Suggestion and Auto-suggestion.* She read it eagerly, was convinced by it, and at once put its suggestions to the practical test, by attempting to banish her unduly emotional feelings for *Y*. The attempt was, in the main, successful. There is only one other dream on this subject recorded. *X* went to stay with *Y* when *S* was also staying in the house. This reawakened her jealousy to some degree. The following dream occurred. It seems to be both the dreamer's *apologia* and an attempt to blacken *S*'s character:

VI. September 16. "There was a king (thought of as Richard II) and he had a Queen and he suspected her intercourse with Launcelot. As a matter of fact their relations were quite innocent. The Queen and Launcelot were walking along the low walls of the Castle and she was protesting to him that the King's suspicions were quite unfounded, and so far from her conduct lending itself to suspicions she never played frivolous American tunes on the piano but only serious ones. She asked whether she could not escape as the walls were so low, but Launcelot said, 'Well, there is the moat' (this in the dream was just a green depression). The King ordered the Queen to be executed and to complete his vengeance placed Launcelot in charge of the execution. He was to provide a nightdress. He got a fine cotton one, but the Queen said she would rather have a thick woollen one. These were spread on a little table, and the Queen sat down on a chair by it. Launcelot told her to cross her arms on it and to put her head down on them. Thus she showed the back of her neck. Launcelot was overcome by affection and bent and kissed it passionately. Then he turned his back and walked away not to see the end. He heard the executioner's boy preparing, and then a loud thud as the axe struck. He turned and wrapped up all that was left (it seemed very small and there was no blood) into a parcel in one of the nightdresses. As soon as this was done, the king returned bringing with him a new bride. The desire to re-marry was the real cause of the execution."

As is not uncommon with *X*, the dream was dreamed twice—once dramatically and the second time as told to *Y*. Between the two repetitions of the dream the outline of the analysis was dreamed. The notes continue: "In the dream I was most clearly identified with Launcelot. *Y* was the queen. The king, though he did not appear, was *S*. I believe the last sentence only occurred in the second version of the dream when it was told. I had been reading *Shakespeare's Workmanship* by 'Q.' in the train and previously *Cymbeline, Pericles* and others of Shakespeare's plays. I had thought a certain amount on the character of the mis-judged wife and this seems to have suggested the form of the dream. The king seems to stand for Richard III and the end suggests Henry VIII. The queen's protestations suggest Hamlet, 'Methinks the lady doth protest too much.' Launcelot, of course, suggests Guinevere. The nightdress, the castle walls and spikes all refer to events and places that concern *Y* and myself. The parcel is a direct reference to *S*."

From this time the friendship has continued unchanged, and *X* seems to have largely solved her problems in connexion with it. At any rate she has ceased to dream on the subject.

(*Manuscript received* 2 *March* 1922.)

MOTOR CAPACITY WITH SPECIAL REFERENCE TO VOCATIONAL GUIDANCE.

By B. MUSCIO.

(*A Report to the Industrial Fatigue Research Board.*)

I. *Aim of the investigations here described* (pp. 157–159).

II. *Description of the experiments* (pp. 159–165).
  (*a*) *Subjects* (p. 159).
  (*b*) *'Motor' and other tests used in the experiments* (pp. 159–164).
  (*c*) *General procedure* (pp. 164, 165).

III. *Results* (pp. 165–171).

IV. *Discussion* (pp. 172–184).

V. *Conclusions* (p. 184).

I. Aim of the Investigations here Described.

In industry as it exists to-day, there are 'occupations' in which the principal activity is the repeated performance of some simple movements or cycle of movements. The making of such movements often requires little strength, and also, since practice tends to produce automatism, little intelligence. On the other hand, the predominant activity in other occupations is *mental*. It is therefore natural to make a broad division among occupations, putting on the one side all those in which the chief emphasis is upon *motor*, and on the other all those in which the chief emphasis is upon *mental* capacity. (There will be an intermediate class of occupations, namely those which make considerable demands upon capacities of both sorts.)

Now it might be argued, on the basis of common observation, that individuals are of predominantly motor or of predominantly mental 'type.' And the suggestion thus arises that such a 'typical' difference between persons, if it exists, might well play a somewhat fundamental part in vocational guidance. The question whether it exists or not is therefore important. But even if there is no such 'typical' difference between persons; even if, contrary to a fairly common assumption, there are not *two* 'general factors,' one mental and one motor; vocational guidance must take account of the distinction between mental and motor capacities. For whether there is a general motor factor or not, some occupations require little of the worker beyond the capacity to make fairly 'simple' movements (a capacity that may appropriately be called motor); while other occupations require, above all, mental capacities or 'intelligence,' and make practically no demands upon the bodily musculature.

One result established by the vast amount of research into 'intelligence' is that mental tests *tend* to correlate positively, and that *many* mental tests, the specifically 'intelligence' tests, tend to correlate highly as well as positively. Apart from any theory as to how this correlation is caused, the fact itself may be taken as the interpretation of the term 'general mental ability' or 'general intelligence.' The important fact for vocational guidance is that 'intelligence' tests tend to correlate both positively and highly; which corresponds to the industrial fact that persons who can efficiently fill one position in which the chief requirement is 'general intelligence' will probably efficiently fill any other position in which the chief requirement is 'general intelligence.' Hence, we do not appear to need *specific* 'mental' tests for different 'intellectual' occupations; that is, occupations in which 'general intelligence' is required more than anything else. What we want to know with regard to such occupations is the *degree* of 'general intelligence' required; and the *same* 'intelligence' tests may then be used to determine native fitness for *all* such occupations.

In psychological literature there are no data concerning motor tests which suggest a similar practical policy as regards motor occupations. But the number of investigations into 'motor capacity' has been extremely small—utterly incomparable with the number of investigations into 'intelligence.' It seems therefore important that attempts should be made to determine the nature of 'motor capacity.' It would certainly be highly significant for vocational guidance if it were shown that there is a general motor factor in the sense in which there clearly is a general mental factor, namely, in the sense that motor tests tend to correlate positively and highly. If this were so, we should be able, by means of suitable motor tests, to detect the person of good 'general motor capacity,' just as now, by means of suitable 'intelligence' tests, it is possible to detect the person of great 'general mental capacity.' Vocational guidance, in such circumstances, would not require a theory of 'general motor capacity' or a theory of 'general mental capacity.' The basis of its procedure would be the high positive inter-correlation between various motor tests, on the one hand, and between various mental tests on the other.

The question considered in this report may now be stated: what are the inter-relations of different motor capacities?

II. DESCRIPTION OF THE EXPERIMENTS.

(*a*) *Subjects.* The subjects of the experiments fall into five groups, which will be referred to as Group A, Group B, Group C, Group D, and Group E[1].

Group A consisted of 20 boys, aged about 15, who were pupils at a central school.

Group B consisted of 16 girls, aged about 13½, who were pupils at an elementary school.

Group C consisted of 12 Cambridge undergraduates, 6 men and 6 women.

Group D consisted of 20 girls, aged about 13, ten of whom were pupils at an elementary school and ten pupils at a central school.

Group E consisted of 20 women medical students (attending the London Royal Free Hospital Women's Medical School).

(*b*) *'Motor' and other tests used in the experiments.* In attempting to determine what relations exist between motor capacities, it is obviously important that the motor tests used should be as simple as possible. If they are complex, their performance will almost certainly involve a mental element, and, since mental capacities tend to correlate positively, this would tend to produce some positive intercorrelation, even if none existed between the motor capacities themselves. It is, of course, not possible wholly to eliminate the mental factor from motor tests (except for the behaviourist); but one can eliminate the 'intelligence' factor, if intelligence be conceived as essentially the capacity which deals with new situations, or, in general, with problems. It is not contended that the motor tests used in the present experiments were perfect in this respect; but most of them were relatively simple, and the chief factor involved in the performance of most of them seemed to be motor.

In what follows, any test which was carried out twice, once with the right hand and once with the left, will be reckoned as *two* tests. Understanding this, 16 simple motor tests were used in these experiments, though all 16 were not used in each group of subjects. They were in every case given individually. They were designed to test accuracy and speed of movements made with the hands, arms, and wrists, as well as various sorts of 'deftness'; and in one group a strength test was used. All tests were carried out with the right hand only, except where the contrary is stated. The tests given to the various groups of subjects were as follows. (A reaction-time test was included among the tests given to Group A. It might be said that such a test, at any rate, is not a test of 'motor capacity' simply, even if it involves a motor capacity. Anyone who feels this difficulty may neglect the results for this test; nothing depends upon them.)

---

[1] I wish to thank Miss S. C. M. Sowton, Miss Mary Sturt, and Mr J. S. Rowntree for giving the tests to Groups B, D, and C respectively.

Tests used in Group A.

(1) *An Aiming test.* (The Whipple aiming blank as given in G. M. Whipple's *Manual of Mental and Physical Tests*, 2nd edition, 1914, Vol. I, p. 149, was used. The blank was placed flat on a table before the seated subject. Thirty shots (three times round the blank) were made in time with the beats of a metronome set at 120. Errors were scored in mm.)

(2) *Tapping.* (At greatest possible rate, with the arm free from the shoulder. A Veeder mechanical counter was used for recording scores.)

Tapping was carried out with each hand separately, and so is reckoned as two tests.

(3) *Tracing.* (Attempting to draw a pencil point between two lines about 2 mm. apart without touching either. The path included frequent turnings at right angles. The blank was placed flat upon a table before the seated subject; whose arm was free from the shoulder. The rate of movement was kept fairly constant for all subjects by preliminary practice for rate alone.)

(4) *A simple Form Board.* (This was so simple as to involve no *intellectual* problem. The test involved chiefly 'deftness' of movement in putting the blocks into their holes. The time taken to do this was used as the score—the shorter the time, the better the score. A standard set of positions for the blocks relative to the subject was of course adopted.)

(5) *Putting Matches into a Match box.* (Thirty matches were laid on a table in standard positions, no two touching, and the subject picked these up, one at a time with the right hand, put them, in proper order—heads all one way—into an ordinary match box held in the left hand, and then closed the box. The score was the time taken—the shorter the better.)

(6) *Steadiness.* (Holding a thin metal rod inside a small circular hole

in a metal plate without allowing it to touch the circumference. The score was the number of touches in five trials of 15 seconds each. Arm free from shoulder. Metal plate perpendicular in front of the subject.) This test was carried out with the right hand and the left hand separately and so is reckoned as two tests.

(7) *Simple Auditory Reaction-time.* (Time measured with Hipp's Chronoscope, the sound being given by means of a sound hammer in circuit with the chronoscope.)

(8) '*Total Strength.*' (Measured by the Martin 'short test,' which will be found fully described in the Industrial Fatigue Research Board's *Report*, No. 16, pp. 41–43.)

Tests used in Group B.

(1) *Tapping.* (As described above; carried out with right hand and left hand separately and hence reckoned as two tests.)

(2) *Aiming.* (Nine holes, gradually decreasing in diameter, were drilled in a metal plate on the circumference of an imaginary circle of about 1½ inches radius. The plate was fixed in front of the subject so that its plane was at right angles to the line of vision. The subject rested the elbow of the arm that was being used in carrying out the test on the table, and endeavoured to put the end of a thin metal rod through each hole in succession without touching the circumference. The thrusts were made in time with the beats of a metronome set at 60, the subject thrusting at a hole with one beat and withdrawing at the next, and so on. The starting point for the thrusts was such that the inserting end of the metal rod was about two inches in front of the plate, which was raised or lowered according to the length of a subject's forearm.

Beginning at a signal with the largest hole, the subject proceeded rhythmically until contact was made in some hole, which was the signal for the end of that trial. The test consisted of seven such trials, of which the first two were treated as 'practice' trials and not counted. Contact was recorded electrically. The holes were numbered 1 to 9 and the score used was the average number of holes passed before contact in the last five trials of the test.)

This test was made with the right and also with the left hand, and is therefore reckoned as two tests.

(3) *Match Stick Insertion.* (This consisted in taking match sticks one at a time from a box in which they were lying together parallel, and putting them into a row of small holes in a board. The distance over which the hand had to travel from the box to the holes was about six

inches. The score was the number of match sticks inserted in 30 seconds. Cf. Industrial Fatigue Research Board's *Reports*, No. 12, p. 30, and No. 16, p. 10.)

This test was made (1) with right hand alone, (2) with left hand alone, and (3) with both hands together; and is therefore reckoned as three tests. In the last case (3), one hand did not feed the other: each hand proceeded exactly as in (1) and (2), the difference being that the hands now worked simultaneously.

The *Match Stick Test* has been used industrially by F. Kemble (see his *Choosing Employees by Test*, Ch. II, Engineering Magazine Co. 1917).

Tests used in Group C.

(1) *Tapping*. (As described above.)

(2) *Aiming*. (As used in Group A, except that the targets here used consisted of concentric circles with a black bull's eye. The diameter of the bull's eye was 2 mm.; from the circumference of this to the circumference of the first circle, 1 mm., and from the circumference of any circle to that of the next, 1 mm. There were nine circles. This device was designed to facilitate scoring; it allows one to score accurately to half a mm., without the use of a rule. The scoring of the standard aiming blank, on which the targets are crosses, is very laborious.)

(3) *Match Stick Insertion*. (In four forms. In one form it was identical with the test used in Group B. In a second form, the box containing the match sticks was placed at arm's length. These will be referred to as the 'near' form and 'distant' form respectively. A third form consisted in the subject picking up the match sticks with the left hand and feeding them to the right which inserted them into the holes, the box being at the distant position. This is called the 'feeding' test. The fourth form consisted in picking up three match sticks at a time with the right hand, and inserting all three together into larger holes. This test is called 'Three Match Sticks.' In each case, the score used was the number of match sticks inserted in 30 seconds.)

The four forms of the match stick insertion test here used are reckoned as four tests.

(4) *Wrist Movement*. (The subject placed the right hand, palm downwards upon a horizontal board, with the wrist between two adjustable upright wooden rods so arranged that the hand could move to right or left by movement of the wrist joint, but the wrist itself could not move either to right or left.

About six inches in front of the upright rods, to right and left, two nails were driven into the board to within half-an-inch of their heads. The line joining them was at right angles to the direction of the forearm. The subject loosely held with his thumb and first and second fingers a thread running freely from a reel supported above, the end of the thread being fastened to one of the nails. The test consisted in winding three metres of thread round the two nails as quickly as possible, the path of the movement to and fro forming, relatively to the subject, an 8 on its side: that is, the thread was first carried round one nail, then round the other, then back again, and so on. The score used was the time taken.)

(5) *Pursuit Pendulum.* (This test, invented by W. R. Miles, is described fully by its inventor in *The Psychological Review*, 1920, XXVII, 361–376. It consists essentially of a pendulum having a period of about one second, through a nozzle at the lower end of which water is led from a reservoir higher up. The subject tries to catch this water in a test tube while the pendulum swings. As here used, the experimenter released the pendulum, which was held by a catch on the extreme left, after giving a signal, and the subject caught as much water as he could in a test tube during one forward and backward movement. Twenty such catches constituted one trial, and the score used was the number of c.c. caught. The foot of the pendulum passed through a distance of a little over 30 inches in one swing. The diameter of the nozzle was 3 mm. and that of the test tube about $\frac{5}{8}$ths of an inch.)

Tests used in Group D.

(1) *Tapping.* (As above described.)

(2) *Aiming.* (With the targets of concentric circles: otherwise as in the A group.)

(3) *Match Stick Insertion.* (As described above: match box in the 'near' position.)

Tests used in Group E.

The tests used with the E group were 'mental' tests. The results obtained with these tests are given for purposes of comparison with the results obtained with the 'motor' tests above described. Three mental tests were used with this group. They will be referred to as T_1, T_2, and T_3.

(1) T_1 consisted of a blank containing 40 arithmetical expressions similar to the following: $6 \times 4 \div 8 = 9, 24, 3, 16$. The correct answer was always included among the numbers on the right-hand side of the expression and had to be underlined. Time allowed, 45″.

(2) T_2 was a 'number group checking' test similar to the Woodworth and Wells test. The groups to be underlined were those each containing a 1, 3, and 7. Time allowed, 75″.

(3) T_3 consisted of a page of digits, the task being to put a circle round every 5, a vertical stroke through every 3, and a horizontal stroke through every 6. Time allowed, 120″.

These three tests were not given individually, but to all 20 subjects at the same time.

(*c*) *General Procedure.* The tests given to any one group were correlated with one another, the formula used throughout being

$$r = 1 - \frac{6S\,(d^2)}{n\,(n^2 - 1)}.$$

A was the first group tested. Here a good adjustment practice trial with the tests was allowed before the experimental trial proper was made. The coefficients of correlation obtained from this group of subjects were generally non-significant (see Table A below, p. 165); which suggested that the functions tested were independent of one another.

The question then arose whether the smallness of these coefficients might not be due, in some degree at least, to the fact that the subjects had had very little practice in the tests. It therefore seemed worth while to have motor tests carried out as many times as possible, in order to determine what changes, if any, would appear in the inter-correlation coefficients at more advanced stages of practice. There is evidence that the correlations between mental tests tend to increase with practice, as will be pointed out more fully below (p. 178), and it is therefore an interesting question, with reference to the problem of the constitution of 'motor capacity,' whether or not the same is true of motor tests.

Accordingly, in Groups B, C, D, and E, the tests were carried out as many times as was practically possible, and the data given below for these groups include correlation coefficients at different practice stages. In addition to an adjustment practice trial, the number of times the tests were given to the different groups was as follows:

Group	*No. of times tests were made*
A	1
B	16 (1 trial a day, 4 days a week)
C	6 (1 trial a week)
D	24 (2 trials a week)
E	36 (all in one week, at intervals of an hour on five days)

Even in the case of Groups D and E, practice effects were never eliminated, and consequently the data here presented can hardly suggest anything as to what is likely to happen in the *final* stages of practice.

III. Results.

(1) *Group A.* The coefficients of correlation between the different tests given to Group A are shown in Table A.

Table A.

Showing correlation coefficients between the motor tests given to Group A—20 subjects.

Tests		Aiming	Tapping		Tracing	Form board	Filling match box	Steadiness		Auditory reaction time	'Total strength'	Average coefficient of each test with all other tests
			R. hand	L. hand				R. hand	L. hand			
Aiming		—	·24	·08	−·05	−·09	−·24	−·06	−·08	−·24	−·02	−·051
Tapping	R. hand	·24	—	·14	−·17	·09	−·04	·18	·08	·06	·01	·026
	L. hand	·08	·14	—	·04	−·17	−·14	·14	·02	·14	−·16	·010
Tracing		−·05	−·17	·04	—	−·05	·31	·15	·31	·07	−·14	·052
Form board		−·09	·09	−·17	−·05	—	·16	−·04	·00	−·07	·07	·011
Filling match box		−·24	−·04	−·14	·31	·16	—	·00	·18	−·08	·07	·024
Steadiness	R. hand	−·06	·18	·14	·15	−·04	·00	—	·40	·06	−·19	·071
	L. hand	−·08	·08	·02	·31	·00	·18	·40	—	−·04	−·11	·084
Auditory reaction time		−·24	·06	·14	·07	−·07	−·08	·06	−·04	—	−·27	−·041
'Total strength'		−·02	·01	−·16	−·14	·07	·07	−·19	−·11	−·27	—	−·082

(2) *Group B.* The coefficients of correlation between the different tests given to Group B are shown in Tables B_1 to B_5.

Table B_1 gives the coefficients between the tests on their first performance (after the adjustment practice trial)—trial No. 1.

Table B_2 gives the coefficients between the tests at the different practice stages represented by trials 1 to 3, 4 to 6, 7 to 9, 10 to 12, and 13 to 15; the scores used as the basis of correlation being the *average* scores in trials 1 to 3 (inclusive), 4 to 6 (inclusive), and so on. This table also gives the average coefficient of each test with every other at the practice stages dealt with.

Table B_3 summarises Table B_2; it gives the *average* coefficients between each pair of tests, these averages being derived from the five individual coefficients obtained from the scores in trials 1 to 3, 4 to 6, etc.

Table B_4 gives the coefficients between the tests on their sixteenth performance—the last.

Table B_5 gives the average coefficients of *all* the tests with one another at each of the five practice stages represented by trials 1 to 3, 4 to 6, etc.

Table B₁.

Showing correlation coefficients between the motor tests given to Group B—16 *subjects—on their first performance* (*after an adjustment practice trial*):

Tests		Match Stick Insertion			Tapping		Aiming		Average coefficient of each test with all other tests
		Right hand	Left hand	Both hands	Right hand	Left hand	Right hand	Left hand	
MatchStick Insertion	R. hd	—	·21	·34	·10	·21	·00	−·09	·13
	L. hd	·21	—	·08	·09	·27	·20	·16	·17
	Both hds	·34	·08	—	·03	·13	−·12	−·27	·03
Tapping	R. hd	·10	·09	·03	—	·20	·30	·25	·16
	L. hd	·21	·27	·13	·20	—	−·16	·04	·11
Aiming	R. hd	·00	·20	−·12	·30	−·16	—	·33	·09
	L. hd	−·09	·16	−·27	·25	·04	·33	—	·07

Table B₂.

Showing correlation coefficients between the motor tests given to Group B, based upon the average scores in trials 1 *to* 3 (*incl.*), 4 *to* 6, 7 *to* 9, 10 *to* 12, *and* 13 *to* 15 (*after the adjustment practice trial*). *In each of the compartments of this table the uppermost coefficient is that obtained on the basis of the average scores in trials* 1 *to* 3, *the second is that obtained from trials* 4 *to* 6, *and so on, the bottom coefficient being that obtained from trials* 13 *to* 15.

Tests		Match Stick Insertion			Tapping		Aiming		Average coefficients of each test with all the others in trials 1 to 3, 4 to 6, etc.
		Right hand	Left hand	Both hands	Right hand	Left hand	Right hand	Left hand	
Match Stick Insertion	Right hand	—	·220	·458	·129	·200	−·100	−·037	·145 (1– 3)
		—	·410	·290	·290	·280	−·080	−·020	·190 (4– 6)
		—	·400	·240	−·010	·350	−·047	·047	·163 (7– 9)
		—	·517	·317	·140	·188	−·220	−·140	·134 (10–12)
		—	·458	·480	·047	·200	−·400	−·100	·114 (13–15)
	Left hand	·220	—	·530	·090	·480	·340	·240	·320 (1– 3)
		·410	—	·388	·210	·300	·080	·048	·239 (4– 6)
		·400	—	·035	·047	·270	−·090	·012	·112 (7– 9)
		·517	—	·530	·230	·160	−·010	·080	·251 (10–12)
		·458	—	·150	·140	·270	−·047	·000	·162 (13–15)
	Both hands	·458	·530	—	·035	·176	−·140	−·100	·160 (1– 3)
		·290	·388	—	·090	·047	−·140	−·100	·096 (4– 6)
		·240	·035	—	·000	·070	−·300	−·051	−·001 (7– 9)
		·317	·530	—	·058	·000	−·170	·023	·126 (10–12)
		·480	·150	—	−·010	·110	−·480	−·330	−·010 (13–15)
Tapping	Right hand	·129	·090	·035	—	·258	·440	·220	·195 (1– 3)
		·290	·210	·090	—	·420	·129	·100	·206 (4– 6)
		−·010	·047	·000	—	·540	·220	·110	·151 (7– 9)
		·140	·230	·058	—	·570	·150	−·220	·160 (10–12)
		·047	·140	−·010	—	·500	·220	−·051	·141 (13–15)
	Left hand	·200	·480	·176	·258	—	−·020	·070	·194 (1– 3)
		·280	·300	·047	·420	—	·210	·220	·246 (4– 6)
		·350	·270	·070	·540	—	·160	·110	·250 (7– 9)
		·188	·160	·000	·570	—	·129	−·300	·124 (10–12)
		·200	·270	·110	·500	—	·100	·000	·200 (13–15)
Aiming	Right hand	−·100	·340	−·140	·440	−·020	—	·400	·150 (1– 3)
		−·080	·080	−·140	·129	·210	—	·630	·136 (4– 6)
		−·047	−·090	−·300	·220	·160	—	·680	·104 (7– 9)
		−·220	−·010	−·170	·150	·129	—	·280	·026 (10–12)
		−·400	−·047	−·480	·220	·100	—	·530	−·010 (13–15)
	Left hand	−·037	·240	−·100	·220	·070	·400	—	·132 (1– 3)
		−·020	·048	−·100	·100	·220	·630	—	·146 (4– 6)
		·047	·012	−·051	·110	·110	·680	—	·151 (7– 9)
		−·140	·080	·023	−·220	−·300	·280	—	−·046 (10–12)
		−·100	·000	−·330	−·051	·000	·530	—	·030 (13–15)

Table B_3.

Showing the average coefficients between the motor tests given to Group B, these averages being derived from the individual coefficients obtained from the average scores in trials 1 *to* 3, 4 *to* 6, 7 *to* 9, 10 *to* 12, *and* 13 *to* 15 (*after the adjustment practice trial*): *that is, the coefficients in the compartments of this table are the averages of the five coefficients in the corresponding compartments of Table B_2.*

Tests		Match Stick Insertion			Tapping		Aiming		Averages (of the averages)
		Right hand	Left hand	Both hands	Right hand	Left hand	Right hand	Left hand	
MatchStick Insertion	R. hd	—	·401	·357	·119	·243	−·169	−·050	·150
	L. hd	·401	—	·327	·143	·296	·055	·076	·216
	Both hds	·357	·327	—	·035	·080	−·250	−·110	·073
Tapping	R. hd	·119	·143	·035	—	·457	·232	·119	·184
	L. hd	·243	·296	·080	·457	—	·115	·050	·207
Aiming	R. hd	−·169	·055	−·250	·232	·115	—	·504	·081
	L. hd	−·050	·076	−·110	·119	·050	·504	—	·098

Table B_4.

Showing correlation coefficients between the motor tests given to Group B on their last performance (*trial No.* 16).

Tests		Match Stick Insertion			Tapping		Aiming		Average coefficient of each test with all other tests
		Right hand	Left hand	Both hands	Right hand	Left hand	Right hand	Left hand	
MatchStick Insertion	R. hd	—	·43	·27	−·05	·28	−·10	·17	·17
	L. hd	·43	—	·01	−·11	·20	−·20	−·11	·04
	Both hds	·27	·01	—	·16	·41	−·08	·23	·17
Tapping	R. hd	−·05	−·11	·16	—	·46	·23	·27	·16
	L. hd	·28	·20	·41	·46	—	−·04	·15	·24
Aiming	R. hd	−·10	−·20	−·08	·23	−·04	—	·35	·03
	L. hd	·17	−·11	·23	·27	·15	·35	—	·18

Table B_5.

Showing the average inter-correlations of all the tests given to Group B at the five practice stages represented by trials 1 *to* 3, 4 *to* 6, 7 *to* 9, 10 *to* 12, *and* 13 *to* 15; ·185, *for instance, is the average of the coefficients between every pair of tests in trials* 1 *to* 3; ·181 *the average of the coefficients between every pair of tests in trials* 4 *to* 6; *and so on.*

Practice stage	1	2	3	4	5
No. of trial	1–3	4–6	7–9	10–12	13–15
Average coefficient of all tests with all others	·185	·181	·133	·110	·085

Table C_1.

Showing correlation coefficients between the motor tests given to Group C—12 adults—on each of the six performances (after the adjustment practice performance). In each of the compartments of this table the uppermost coefficient is that obtained at the first performance of the tests, the second is that obtained at the second performance, and so on, the bottom coefficient being that obtained at the sixth (last) performance of the tests.

Tests		Aiming	Tapping	Wrist movement	Pendulum	Match Stick Insertion				Average coefficient of each test with all the other tests at each performance
						Near position	Distant position	3 Match sticks	Feeding test	
Aiming		—	·19	·08	·34	·32	·54	·32	·14	·276 (1st perf.)
		—	·78	·12	·18	·50	·38	·46	·64	·437 (2nd „)
		—	·70	·52	·30	·30	·46	·50	·34	·446 (3rd „)
		—	·78	·38	·30	·36	·52	·48	·40	·446 (4th „)
		—	·36	·60	·63	·18	·06	·22	·12	·310 (5th „)
		—	·50	·28	·13	–·04	·44	·56	·40	·327 (6th „)
Tapping		·19	—	–·18	·20	·30	·65	·64	·46	·323 (1st „)
		·78	—	–·28	·12	·30	·60	·45	·56	·361 (2nd „)
		·70	—	·14	·34	·36	·70	·48	·48	·457 (3rd „)
		·78	—	·30	·50	·53	·56	·30	·22	·456 (4th „)
		·36	—	·02	·36	·28	–·08	·44	·04	·203 (5th „)
		·50	—	–·24	–·16	–·44	·10	·12	–·16	–·040 (6th „)
Wrist Movement		·08	–·18	—	·50	·06	·09	·07	·16	·111 (1st „)
		·12	–·28	—	·49	·27	–·40	–·06	·05	·027 (2nd „)
		·52	·14	—	·66	–·06	·18	·38	·46	·326 (3rd „)
		·38	·30	—	·37	·52	–·10	·07	·50	·291 (4th „)
		·60	·02	—	·36	·48	·32	·34	·54	·380 (5th „)
		·28	–·24	—	·57	·62	–·06	·42	·54	·304 (6th „)
Pendulum		·34	·20	·50	—	·50	·42	·06	·40	·346 (1st „)
		·18	·12	·49	—	·68	·06	·24	·68	·350 (2nd „)
		·30	·34	·66	—	·30	·29	·54	·56	·427 (3rd „)
		·30	·50	·37	—	·73	·55	·08	·26	·399 (4th „)
		·63	·36	·36	—	·50	·04	·22	·28	·341 (5th „)
		·13	–·16	·57	—	·64	·14	·34	·42	·297 (6th „)
Match Stick Insertion	Near position	·32	·30	·06	·50	—	·52	·54	·58	·403 (1st „)
		·50	·30	·27	·68	—	·24	·52	·72	·461 (2nd „)
		·30	·36	–·06	·30	—	·53	·64	·26	·333 (3rd „)
		·36	·53	·52	·73	—	·35	·12	·40	·430 (4th „)
		·18	·28	·48	·50	—	·61	·64	·76	·493 (5th „)
		–·04	–·44	·62	·64	—	–·12	·51	·48	·236 (6th „)
	Distant position	·54	·65	·09	·42	·52	—	·63	·54	·484 (1st „)
		·38	·60	–·40	·06	·24	—	·80	·34	·289 (2nd „)
		·46	·70	·18	·29	·53	—	·49	·56	·459 (3rd „)
		·52	·56	–·10	·55	·35	—	·14	·03	·293 (4th „)
		·06	–·08	·32	·04	·61	—	·22	·66	·261 (5th „)
		·44	·10	–·06	·14	–·12	—	·28	·01	·113 (6th „)
	3 Match sticks	·32	·64	·07	·06	·54	·63	—	·38	·377 (1st „)
		·46	·45	–·06	·24	·52	·80	—	·52	·419 (2nd „)
		·50	·48	·38	·54	·64	·49	—	·52	·507 (3rd „)
		·48	·30	·07	·08	·12	·14	—	·30	·213 (4th „)
		·22	·44	·34	·22	·64	·22	—	·50	·369 (5th „)
		·56	·12	·42	·34	·51	·28	—	·75	·426 (6th „)
	Feeding test	·14	·46	·16	·40	·58	·54	·38	—	·380 (1st „)
		·64	·56	·05	·68	·72	·34	·52	—	·501 (2nd „)
		·34	·48	·46	·56	·26	·56	·52	—	·454 (3rd „)
		·40	·22	·50	·26	·40	·03	·30	—	·301 (4th „)
		·12	·04	·54	·28	·76	·66	·50	—	·414 (5th „)
		·40	–·16	·54	·42	·48	·01	·75	—	·349 (6th „)

(3) *Group C.* The correlation coefficients between the different tests given to Group C are shown in Tables C_1 to C_3.

Table C_1 gives the coefficients between *each pair* of tests at each of the six performances of them (after the adjustment practice trial).

Table C_2 gives the average coefficients between *each pair* of tests, these averages being derived from the six coefficients obtained at each of the six performances; that is, this table summarises Table C_1.

Table C_3 gives the average coefficients of *all* the tests with one another at each of the six performances.

Table C_2.

Showing the average coefficients between each pair of motor tests given to Group C, these averages being derived from the individual coefficients obtained at each of the six test performances (after the adjustment practice performance); that is, the coefficients in the compartments of this table are the averages of the coefficients in the corresponding compartments of Table C_1.

						Match Stick Insertion				
Tests		Aiming	Tapping	Wrist movement	Pendulum	Near position	Distant position	3 Match sticks	Feeding test	Averages (of the averages)
Aiming		—	·551	·330	·313	·270	·400	·423	·340	·375
Tapping		·551	—	−·040	·227	·221	·421	·405	·267	·293
Wrist movement		·330	−·040	—	·491	·315	−·011	·203	·375	·238
Pendulum		·313	·227	·491	—	·558	·250	·247	·433	·359
Match Stick Insertion	Near position	·270	·221	·315	·558	—	·355	·495	·533	·392
	Distant position	·400	·421	−·011	·250	·355	—	·427	·357	·314
	3 Match sticks	·423	·405	·203	·247	·495	·427	—	·495	·385
	Feeding test	·340	·267	·375	·433	·533	·357	·495	—	·400

Table C_3.

Showing the average correlation coefficients of all the tests given to Group C with one another at the six practice stages represented by trials 1 *to* 6 (*cf. Table* B_5).

Practice stage	1	2	3	4	5	6
No. of trial	1	2	3	4	5	6
Average coefficient of all tests with all others	·337	·356	·426	·356	·363	·251

(4) *Group D.* The results for this group are given in Tables D_1, and D_2.

Table D_1 gives the correlation coefficients between each pair of tests on all the 24 occasions (after the adjustment practice trial) on which the tests were given; and also the *average* coefficient on each occasion.

Table D_2 gives *average* coefficients for the practice stages represented by trials 1 to 3, 4 to 6, 7 to 9, 10 to 12, 13 to 15, 16 to 18, 19 to 21, and 22 to 24; these *averages* being based upon the average coefficient of the different tests with one another on each three occasions indicated by trials 1 to 3, 4 to 6, etc.

Table D_1.

Showing correlation coefficients between the motor tests given to Group D—20 subjects—on each of the 24 occasions (after the adjustment practice trial) on which the tests were given; and also the average coefficient on each occasion.

Number of trial	Correlation coefficient			Average coefficient
	Tapping and Aiming	Aiming and Match Stick	Tapping and Match Stick	
1	·23	·21	·45	·297
2	·01	−·11	·27	·060
3	·36	−·07	·13	·140
4	·72	·55	·62	·630
5	·20	·18	·02	·133
6	·46	·45	·47	·460
7	·56	·19	·35	·367
8	·45	·19	·33	·280
9	·41	·14	·20	·250
10	·25	·12	·04	·137
11	·46	·29	·20	·250
12	·18	·16	·16	·174
13	·40	−·13	·34	·203
14	·36	·20	·16	·240
15	·08	·19	·60	·290
16	·15	−·16	·16	·050
17	·18	·14	·24	·187
18	·13	−·26	·37	·080
19	·23	−·18	·60	·217
20	·12	−·18	·58	·174
21	−·02	−·20	·50	·094
22	−·34	−·34	·36	−·107
23	·46	·08	·36	·300
24	·31	·26	·48	·350
Averages	·26	·07	·33	·220

Table D_2.

Showing the average correlation coefficients of all the tests given to Group D with one another at the eight practice stages represented by trials 1 *to* 3, 4 *to* 6...22 *to* 24; *the coefficient* ·166, *for the first practice stage, is the average of the first three coefficients in the column headed 'Average coefficient' in Table* D_1, *the coefficient* ·408, *for the second practice stage, is the average for the second three coefficients in the same column, and so on.*

Practice stage	1	2	3	4	5	6	7	8
No. of trial	1–3	4–6	7–9	10–12	13–15	16–18	19–21	22–24
Average coefficient of all tests with all others	·166	·408	·299	·187	·244	·106	·162	·181

(5) *Group E.* The results for this group are shown in Tables E_1 and E_2, which give information similar to that shown (for the D group) in Tables D_1 and D_2.

Table E_1.

Showing the correlation coefficients between the mental tests given to Group E—20 subjects—on each of the 36 *occasions* (*after the adjustment practice trial*) *on which the tests were given; and also the average coefficient on each occasion.*

Number of trial	Correlation coefficients T_1 with T_2	 T_1 with T_3	 T_2 with T_3	Average coefficient
1	·33	·00	·22	·183
2	·08	·30	·56	·313
3	·02	·27	·22	·170
4	−·06	·00	·44	·127
5	·34	·50	·42	·420
6	·64	·32	·34	·433
7	·06	·38	·39	·277
8	−·02	·25	·42	·217
9	·33	·38	·24	·313
10	·16	·38	·28	·273
11	·09	·46	·30	·283
12	·16	·28	·33	·257
13	·20	·32	·04	·187
14	·18	·42	·20	·267
15	·18	·24	·24	·220
16	·12	·18	·28	·193
17	−·02	·33	·00	·103
18	·12	·44	·28	·280
19	·00	·34	·28	·207
20	·11	·32	·60	·343
21	·26	·18	·32	·253
22	−·14	·46	·02	·113
23	·34	·38	·14	·283
24	−·08	·56	·26	·247
25	·14	·64	·38	·387
26	·38	·72	·26	·453
27	·14	·46	·28	·293
28	·36	·20	·26	·273
29	−·02	·54	·30	·273
30	·34	·46	·42	·407
31	·72	·44	·42	·527
32	·26	·46	−·08	·213
33	·36	·56	·32	·413
34	·12	·46	·52	·367
35	−·16	·42	·28	·180
36	·34	·53	·42	·430
Average	·18	·38	·28	·280

Table E_2.

Showing average correlation coefficients between the mental tests given to 20 *young women medical students for the* 12 *practice stages represented by trials* 1 *to* 3 (*after the practice adjustment trial*), 4 *to* 6, 7 *to* 9...34 *to* 36 (*cf. heading to Table D_2*).

Practice stage	1	2	3	4	5	6	7	8	9	10	11	12
Number of trial	1–3	4–6	7–9	10–12	13–15	16–18	19–21	22–24	25–27	28–30	31–33	34–36
Av. coefficient of all tests with all others	·222	·327	·269	·271	·225	·192	·268	·214	·378	·318	·384	·326

IV. DISCUSSION.

(1) The results for the 'motor' tests (given to Groups A, B, C and D) will be considered first. The outstanding feature of these results is the small degree of correlation between the various tests.

Of the coefficients obtained from the A group, the highest, that between steadiness with the right hand and steadiness with the left, is only ·40, which is barely three times its probable error (= ·13); and half of all the coefficients between the different pairs of tests have negative signs.

The results for the B group are in perfect agreement with those for the A group; and to the B group the tests were given 16 times. So far as the intercorrelation of the B group tests is concerned, reference should be made to Table B_3, which gives average coefficients. The three highest coefficients in this Table are between the right and left-hand performances of the same test, these being ·401, ·457 and ·504. The next highest coefficients are between the left-hand and right-hand performances with the both hands performance of the Match Stick Insertion test. Neglecting the coefficients between the right and left-hand performances (and, in the case of the Match Stick Insertion test, the both hand performance) of any test, all the coefficients for this group are non-significant.

Consider next the results for Group D. To this group tests were given 24 times, and the results agree generally with those for Groups A and B. There are, it is true, positive and significant coefficients between certain pairs of tests on certain occasions; but the average coefficients between these tests for all 24 occasions on which they were given, while all positive, are all small; the average coefficient of Tapping with Aiming is ·26, that of Aiming with Match Stick Insertion is ·07, and that of Tapping with Match Stick Insertion is ·33 (see bottom of Table D_1).

With the C group (the smallest of all the groups—12 subjects) the coefficients tend to be higher than with the other groups (see especially Table C_2 which gives average coefficients for this group—the tests having been carried out six times). Even here the highest average coefficient between any pair of tests is only ·558, and more than half the total number of coefficients are under ·360. The fact that the coefficients for this group are rather higher than for the other groups was due to a special characteristic of this (C) group. It contained one subject (a woman who had been a physical drill mistress) who was particularly good at all tests; and it contained as well one subject (a man) who was particularly bad at all the tests. A good illustration of the fact that such a peculiarity of a group may raise coefficients is given by B. R. Simpson[1]. He gave a number of mental tests to a total of 37 subjects, made up as follows: 17 professors and advanced students in Columbia University, and the remaining 20 "selected from men in New York City who had never held any position demanding a high grade of intelligence[2]." Eleven of the latter were "staying at the Salvation Industrial Army Home at the nominal salary of $1 per week in addition to board and room, until work could be secured," and "seven were found in a mission[3]." Coefficients of correlation between the tests were calculated for the group of 37 as a whole, and also for the two sub-groups of 17 and 20 separately. The broad result was that the coefficients for the whole group of 37 were higher than for either sub-group alone; as is perfectly intelligible since the sub-group of 17 would tend to be at the top of the group of 37 in all the tests and the sub-group of 20 at the bottom of it. The bottom of the 'good' group might overlap with the top of the 'poor' group; but otherwise the two sub-groups would be distinct; and in this case tests would yield positive correlations for the whole group even if they yielded zero correlations for each sub-group. The following coefficients illustrate the general tendency of Simpson's results. Hard Opposites, correlated with Memory for Words, yielded coefficients of ·92 for the whole group of 37, ·75 for the sub-group of 17, and ·77 for the sub-group of 20. Again, Hard Opposites and the A test yield coefficients of ·76 for the whole group of 37, ·15 for the sub-group of 17, and ·65 for the sub-group of 20[4].

The fact, then, that in the present C group there was one subject who was generally at the top of the group in every test, and another who was generally at the bottom in every test, is the main cause that the coefficients for this group tend to be rather higher than the coefficients for groups A, B and D; for these three groups showed no such feature as this. That this is the correct explanation can be proved by calculating the coefficients between the tests for the C group, with these two subjects omitted from it, and comparing these coefficients with the corresponding coefficients for the whole group of 12. To illustrate the influence of these two subjects, the coefficients between Aiming and each other test, and between Tapping and each other test, for the first performance of the tests, are given here both for the group of 12 and the group of 10. The coefficients for the group of 12 are taken from Table C_1 (p. 168). Reading in that table from left to right, the coefficient for Aiming with each other test (on their first performance) are as follows (the top row of coefficients): ·19, ·08, ·34, ·32, ·54, ·32, ·14—the average of which is ·276. The corresponding coefficients for the group of 10 are: − ·11, − ·13, − ·07, − ·20, ·30, − ·13, − ·40—the average of which is − ·106. The coefficients for Tapping are (again reading from left to right) for the group of 12: ·19, − ·18, ·20, ·30, ·65, ·64, ·46—the average being ·323; and the corresponding coefficients for the group of 10 are: − ·11, − ·26, − ·07, ·04, ·55, ·59, ·28—the average being ·160. Thus, every single one of these coefficients for the group of 12 has its positive character diminished by taking out of the group the two subjects referred to. With the group of 12 subjects 12 out of the 13 coefficients (given above) are positive, while with the group of ten no fewer than eight are negative.

---

[1] *Correlation of Mental Abilities* (Teachers' College, Columbia University, Contributions to Education, No. 53, 1912.)

[2] *Ibid.* p. 6. [3] *Ibid.* [4] *Ibid.* Table X, p. 58.

The fact that intercorrelations of tests are affected by the nature of the group from which they are derived gives rise to a great difficulty for correlation experiments in psychology, which, however, cannot be discussed here. With regard to our present problem we may say, that the results for all the groups to which motor tests were given, agree in showing that the intercorrelations of motor test performances tend to be non-significant.

The only published investigation dealing specifically with the problem under consideration in this report, is that by F. A. C. Perrin[1]. He gave 17 motor tests (including several which might be said not to be strictly 'motor') to 51 subjects, undergraduates (both sexes) of the University of Texas. Three of the tests used were 'complex' in character—the Bogardus test, card sorting, and a 'co-ordination' test (tracing the perimeter of a quadrilateral with a stylus held in the right hand, and at the same time the perimeter of a triangle with a stylus held in the left hand). The remaining 14 tests were 'tests of elementary motor functions': reaction time, aiming, tracing, tapping, steadiness, etc. The object of the experiment was to obtain evidence as to the nature of 'motor ability.' The results obtained in the present investigations are in general agreement with those given by Perrin. The coefficients between Perrin's tests were non-significant almost without exception—in many cases they were approximately ·00[2]. "It seems quite patent" says Perrin, "that motor ability is not general, but that it is somewhat definitely specialised. As a statement of fact, this means merely that absence of correlation obtained between the scores of the various tests, both complex and simple[3]."

---

[1] "An Experimental Study of Motor Ability," *Journal of Experimental Psychology*, 1921, IV, 24–56.

[2] See Table on p. 45, *ibid.*

[3] *Ibid.* p. 49.

Investigators who have been interested in the relations between the capacities required to perform different tests, have, as a rule, made use of mental tests, and have tended to use chiefly tests of 'intelligence.' Now the typical table of intercorrelations between 'intelligence' tests is very different from Perrin's table of coefficients between motor tests and from the tables given in this report; for 'intelligence' tests tend to yield high positive intercorrelations. Thus, in B. R. Simpson's Table IX[1], which gives the raw coefficients between the mental tests used by him, there is, for the whole group of 37 subjects, a considerable number of coefficients over ·70; and out of a total of 91 coefficients in the table, no fewer than 70 are over ·30. Even granting that Simpson's coefficients were to some extent increased in size in consequence of the special nature of his group of subjects, the results of many investigators agree in showing a definite tendency for the intercorrelations of 'intelligence' tests to be positive and high[2].

It thus seems clear that the inter-relations of the functions involved in the performance of many, though (*vide* results for Group E) not all, *mental* tests are much more intimate than the inter-relations of the functions involved in the performance of many *motor* tests. Hence, while we may significantly speak of *mental capacity*, we should, if we are to speak accurately, use the term *motor capacities*.

(2) Consider now another interesting feature of the motor test results —*the effect of practice in the tests on the intercorrelations between them.* In order to bring out the nature of the present results, reference will first be made to a paper by H. L. Hollingworth[3].

Hollingworth deals with the results of each of six tests which had been given to a group of 13 adult subjects 205 times. Intercorrelations were calculated for five points on the practice curve thus obtained, these points being referred to as the 1st, 5th, 25th, 80th and 205th performances. At each of these five points, the average coefficient of each test with all the others was calculated. The tests used and the broad results are shown in the following table[4]. In this table the basis of correlation for 'trial No. 5' was the 'median of first 5 trials,' for 'trial No. 25' the 'median of trials 20 to 25,' for 'trial No. 80' the 'median for trials 75 to 80,' and for 'trial No. 205' the 'median of trials 200 to 205[5].'

---

[1] *Correlations of Mental Abilities*, p. 56.

[2] Cf., for instance, the relevant figures in Burt's table of coefficients, *This Journal*, 1909–10, III, 177.

[3] "Correlation of Abilities as Affected by Practice" (*Journal of Educational Psychology*, 1913, IV, 405–414).

[4] *Ibid.* p. 408.

[5] *Ibid.* p. 406 and p. 409.

Table H_1.

Hollingworth's Table II.

Showing the average correlation coefficient of each test with all others, at various points in the practice curve.

	Tests						
No. of trial	Adding	Opposites	Colour naming	Choice reaction	Motor co-ordination	Tapping	Final averages
1	·19	·10	·15	– ·07	– ·15	·17	·065
5	·41	·26	·15	·35	·21	·32	·280
25	·50	·35	·43	·27	·03	·35	·320
80	·55	·43	·53	·31	·18	·34	·390
205	·48	·62	·61	·35	·34	·52	·490

Hollingworth sums up his results as follows: "Except in the case of *Choice Reaction* ('discrimination time') the effect of practice is to increase, to a marked degree, the intercorrelations of the various tests[1]."

Now, three of Hollingworth's tests—adding, opposites and colour naming—are 'mental' tests, while the remaining three may be classified as 'motor.' When the results for all six are averaged, as in the column of Table H_1 headed 'Final averages,' the results show a regular increase in intercorrelation with practice. Nevertheless, this increase is much more evident with the three mental tests than with the motor tests; and indeed one of the latter, the choice reaction test, is explicitly mentioned by Hollingworth as an exception to his general statement. But Hollingworth's 'motor coordination' test is as much an exception to his general statement as the choice reaction test. The coefficients for this 'motor coordination' test show an actual decrease in size from the '5th' to the '80th' trial, though the '205th' trial yields the highest coefficient, just as it yields the highest coefficient for the choice reaction test. Hollingworth should have said that both choice reaction and motor coordination were exceptions to his general statement. What he says about 'motor coordination' is this: "In coordination the increase (in the size of the coefficients) is very irregular, but the coefficients show, on the whole, a change from – ·15 at 1st trial to ·34 at the finish[2]." But why distinguish this test from choice reaction, which shows a similar change, except that it is less irregular; a change, namely, from – ·07 at 1st trial to ·35 at the finish? Clearly, neither of these tests exhibits that general increase in the size of coefficients which appears in the column headed 'Final averages' in Table H_1.

Consider now Hollingworth's third motor test—tapping. Of this test, Hollingworth says that the coefficients "increase more slowly (than adding, opposites and colour naming) but no less certainly[1]." On examining the tapping coefficients, however, we find that from the 5th to the 80th trial (Table H_1) the increase in their size amounts to only ·02, which is practically none at all; the appearance of gradual increase from the 1st to the '205th' trial being due to the fact that the coefficient from the 1st trial is smaller than from the '5th,' and the coefficient from the '80th' smaller than that from the '205th.'

---

[1] *Journ. of Educ. Psychol.*, 1913, IV, 408.

[2] *Ibid.* p. 408.

By calculating separately the average coefficients for the mental tests with all tests, and the average coefficients for the motor tests with all tests, it can be seen that the coefficients for the motor tests show no steady increase with practice, whereas the mental tests do. This is seen in Table H_2.

Table H_2.

Showing the average correlation coefficients of Hollingworth's three mental tests with all his tests, and the average correlation coefficients of his three motor tests with all his tests, at various points in the practice curve. This table is obtained by averaging the relevant figures in Table H_1.

Number of trial	Average coefficient for the three mental tests	Average coefficient for the three motor tests
1	·147	–·017
5	·274	·293
25	·427	·217
80	·503	·277
205	·570	·403

We thus see that the average coefficients for the motor tests with all tests show an actual slight decrease from the 5th to the 80th trial.

In Tables H_1 and H_2, however, what is expressed is the tendency of any test (or set of tests) to correlate with all the other tests used, both motor and mental. They do not show what is the tendency of any motor test to correlate with other motor tests simply, nor what is the tendency of any mental test to correlate with other mental tests simply. Information on this point can be obtained from Hollingworth's Table I[2], which gives the correlation of each test with every other at the various practice stages dealt with. Tables H_3 and H_4 are constructed from figures given in Hollingworth's Table I in such a way as to yield this information. The only figures here added are those in the column of averages (right-hand column of tables).

---

[1] *Journ. of Educ. Psychol.*, 1913, IV, 408.

[2] *Ibid.* p. 407.

Table H_3.

Showing the correlation coefficients of each of Hollingworth's mental tests with every other of his mental tests, at various practice stages. (The individual coefficients are taken from Hollingworth's Table I.)

Number of trial	Adding with opposites	Adding with colour naming	Opposites with colour naming	Averages
1	·23	·26	·53	·340
5	·17	–·03	·66	·266
25	·57	·91	·54	·673
80	·63	·85	·68	·702
205	·76	·76	·88	·800

Table H_4.

Showing the correlation coefficients of each of Hollingworth's motor tests with every other of his motor tests at various practice stages. (The individual coefficients are taken from Hollingworth's Table I.)

Number of trial	Tapping with coordination	Tapping with choice reaction	Choice reaction with coordination	Averages
1	–·25	·11	–·08	–·074
5	·50	·23	·33	·387
25	·13	–·05	·35	·143
80	·26	–·08	·30	·160
205	·39	·19	·59	·390

Comparing the column of averages in Table H_3 with the corresponding column in Table H_4, we find considerable difference. In the first place, the coefficients between the mental tests (Table H_3) are considerably larger than those between the motor tests (Table H_4). The average of all the coefficients in the column of averages for the mental tests is ·556, whereas the corresponding average for the motor tests is only ·201. Secondly, there is a continuous increase in the size of the average coefficients between the mental tests with practice, with the exception of the coefficient for the '5th' trial, which is smaller than that for the 1st; while such an increase is not present in the coefficients between the motor tests. Indeed, taking the figures in the column headed 'Averages' in Table H_4, it will be seen that the average coefficient for the '205th' trial (motor tests) is greater by only ·003 than the coefficient for the '5th' trial! It is true that there is an increase in the size of the coefficient from the 1st trial to the '5th'; but after the '5th' trial there is practically no increase at all. In this respect the coefficients for the mental tests are very different.

But further, the coefficients for Hollingworth's '1st trial' are not strictly comparable with those for any other of his trials; for they are based on only one performance of the tests, whereas those for trials 'No. 5,' 'No. 25,' 'No. 80,' and 'No. 205' are each based on the median of five performances. If, then, we strike out Hollingworth's coefficients for his trial No. 1 and compare simply the coefficients from the remaining trials (which are strictly comparable), we see that the coefficients for the mental tests continuously increase with practice, whereas those for the motor tests do not.

The conclusions suggested by Hollingworth's data thus appear to be (1) that practice in mental tests increases 'to a marked degree' their intercorrelations; and (2) that practice in motor tests increases the degree of their intercorrelations only in the early stages of such practice (if at all), and that after a small amount of practice there is no increase in degree of intercorrelation. Whether this general statement is true of all mental and all motor tests is a question which cannot be decided from Hollingworth's data. And the question is further complicated by the fact that (in Hollingworth's results) the coefficients of certain mental tests with certain motor tests also tend to increase with practice. Thus, Opposites and Tapping, and Opposites and Choice Reaction, show an increase in correlation with practice; so do Colour Naming and Tapping, Colour Naming and Motor Coordination, and Colour Naming and Choice Reaction. Adding shows no increase in correlation with any motor test and with both Motor Coordination and Choice Reaction the coefficient for the '205th' trial is not more than half that for the '5th[1].'

We may now consider such of the results obtained in the investigations here reported as are relevant to the present question. They will be found in summarised form in Tables B_5, C_3, D_2, and E_2 (III above). The various tables from which these four tables have been constructed should also be consulted (especially Tables B_2, C_1, D_1 and E_1). The results for the motor tests (Groups B, C and D) will be considered first.

The stages of practice reached in the present investigations are not comparable with the final stages of practice in Hollingworth's experiment; but with such practice as there was in the present investigation there is no tendency whatever for the intercorrelations of the tests to increase, except perhaps during the first few trials. The coefficients in Table B_5 even show a consistent decrease in size over the five practice stages representing 15 performances of the tests. In Table C_3, the average coefficient increases slightly from the 1st to the 3rd trial, but then decreases again; and the average coefficient for the 6th trial (the last) is the smallest of all. In Table D_2 there is an increase in the coefficients from the first to the second 'practice stage' (representing the averages of trials 1 to 3, and 4 to 6 respectively; see column headed 'Average coefficient'); after which there is a considerable decrease in the size of the coefficients, though the results are not regular. At none of the eight practice stages representing altogether 24 trials, does the average coefficient approach that for the second stage. So far as they go, then, these results are in agreement with the results obtained by Hollingworth for motor tests; and as our results were obtained from three groups of subjects, and refer to considerably more tests than were used by Hollingworth, it may be accepted as probable that the intercorrelations of motor tests do not increase with practice, except perhaps during the first few performances. It should be recognised, however, that in the present investigations the amount of practice was small, and can of itself give no indication of what would happen after a much greater amount. We have to rely on Hollingworth's data—at least, for the present—for conclusions as to what is likely to happen in advanced practice stages.

---

[1] Hollingworth's Table I, *Journ. of Educ. Psychol.*, 1913 IV, 407.

That further investigation is required is suggested by the results given above for Group E, which consist of intercorrelations between the mental tests.

The first thing to strike us about these results is that the intercorrelations here are low. Table E_1 is closely comparable with Table D_1; and a comparison shows that the intercorrelations of the three mental tests (Table E_1) are of about the same size as the intercorrelations of the three motor tests (Table D_1). Looking at the figures at the bottom of these two tables, it will be seen that the average intercorrelations of the three mental tests are very slightly the larger; but against this must be placed the fact that the averages for the mental tests are derived from 36 performances of the tests, while the averages for the motor tests are derived from 24. If we calculate the averages for the first 24 performances of the mental tests, we find that they are almost identical with the averages for the 24 performances of the motor tests; T_1 with $T_2 = \cdot 24$, T_1 with $T_3 = \cdot 32$ and T_2 with $T_3 = \cdot 12$ (the corresponding coefficients for the motor tests are ·26, ·33 and ·07). So far, then, as these mental tests are concerned, the capacities to perform them seem to be nearly independent of one another.

The second striking feature of the mental test results is that during the first 24 performances of them there is no more increase in their intercorrelations than there is in the intercorrelations of the motor tests during their 24 performances (cf. Tables D_2 and E_2). After their 24th performance the mental tests exhibit some increase in the degree of this intercorrelation; but if we had only the results for their first 24 performances, we should have no reason to expect this increase, except on the basis of Hollingworth's data. As it is, the present mental test results are in general accord with Hollingworth's results, the average coefficient of all tests with all others for the 11th practice stage (the second last, Table E_2) being higher than that for any preceding practice stage.

The question here arises: what certainty, then, is there that the intercorrelations of the motor tests given in the present investigations would not have shown an increase in size at later practice stages than those reached? Supposing, for instance, that the motor tests given to Group D had been carried out 36 times instead of 24, what ground is there for inferring that between the 24th and the 36th performance of them, their intercorrelations would not have increased in size, as happened with the mental tests given to Group E? To such questions the general answer is that we can infer nothing at all as to what will happen in the final stages of practice in motor tests from the present experiments alone. It is true that there is no sign of any increase in the size of the intercorrelations up to the furthest practice stage reached, in the present investigations, with motor tests; but nothing can be inferred from this as to what might happen later, as is clear from the fact that the three mental tests given to Group E show an exactly similar feature up to their 24th performance, and yet show an increase, no doubt small, in their degree of intercorrelation later. From Hollingworth's results, however, it may be inferred that intercorrelations of motor tests are not likely to increase in size with practice, though this inference is little more than a hypothesis to be tested further[1].

In order to explain increase, with practice, in the degree of intercorrelation of tests (where it occurs), Hollingworth has recourse to the hypothesis of a 'general factor.' Decreasing variability of performance with practice, and also the tendency of certain tests, such as opposites, adding and colour naming to become more alike with practice, may, he thinks, play some part in producing the greater degree of intercorrelation; but the 'general ability' factor is supposed to be its chief cause.

He says: "If there is such a thing as 'general ability' or 'general intelligence' we should expect all samplings of that ability to correlate more and more, the more the measures came to be 'true samplings.' We might, indeed, expect to find evidences of this general ability only when measuring the 'ultimate capacity' of the individuals concerned. The momentary ability revealed in initial trials, or even in the first half-dozen trials in a given set of tests might well be expected to show only low degrees of correlation. These would not be measures of ultimate capacity, but would be highly determined by previous practice, momentary attitude and initial method of attack. They would, in short, be samplings only of momentary ability, not of final capacity[1]." This reasoning seems plausible with reference to such tests as show increased intercorrelation with practice. But what shall be said of those that do not?

---

[1] A small amount of data bearing on the general question under discussion is given by Miss G. I. Strickland in "The Influence of Practice on the Correlation of Abilities" (*Journal of Educational Psychology*, 1918, IX, 393–399). Miss Strickland had 15 subjects and used five tests—all 'mental' except one, tapping—and gave the tests 22 to 29 times. The intercorrelation results are not very conclusive.

Assuming the motor tests used in the present investigations to be representative, we seem driven to the conclusion that performance in motor tests is not appreciably affected by any 'general factor'; neither by the factor usually referred to as 'general intelligence,' nor by a factor which might be called 'motor dexterity' or 'practical ability.' If there is a factor of 'general intelligence,' it seems likely that it would have some influence on every activity of an individual; but its influence on mental activities would appear to be much greater as a rule (though apparently not always) than its influence on motor activities, with regard to which special factors seem to neutralise or at any rate to mask its influence.

If, on the other hand, there were a general motor factor, muscular or neuro-muscular in nature, we should expect it to influence the performances of all motor tests, in such a way as to produce positive correlation between them. The absence, even after a certain amount of practice, of appreciable positive intercorrelation (if it is general for all motor tests) is evidence either that there is no general motor factor, or that, if there is, its effect on the performance of motor tests is largely neutralised by special factors (possibly local neuro-muscular mechanisms). The carrying out of certain mental tests will also, apparently, depend primarily upon certain special capacities.

For a further discussion of various hypotheses concerning the nature of 'motor ability' the reader is referred to the above-mentioned paper by F. A. C. Perrin (pp. 49–56).

(3) In the foregoing discussion nothing has been said concerning the 'reliability' of the motor tests used in the present investigations. If the 'reliability' of these tests were low, the fact that their intercorrelations are negligible would have much less significance than if it

---

[1] *Journ. of Educ. Psychol.*, 1913, IV, 411.

were high. As the results for all the groups are very similar, coefficients of 'reliability' have not been calculated for all of them, but for Group D only, as being the largest 'motor test' group. These coefficients are relatively high, as will be seen from the figures now to be given (Tables R_1 and R_2). Two 'reliability' coefficients were calculated for each of the three tests given to Group D, and these may be said to indicate initial and final 'reliability' respectively—'final' here having reference to the practice stage reached in the tests and not to what might be called ultimate 'reliability.' The initial 'reliability' coefficient is the coefficient of the combined score in the first and fourth performances of a test correlated with the combined score in the second and third; the final 'reliability' coefficient is the coefficient of the combined score in the twenty-first and twenty-fourth performances correlated with the combined score in the twenty-second and twenty-third. In addition, the first performance of each of these tests was correlated with its second, the second with the third, the third with the fourth and so on up to the ninth performance. These coefficients also are fairly high.

Table R_1.

'Reliability' coefficients for the three motor tests given to Group D.

Tests	Initial 'reliability' coefficients	Final 'reliability' coefficients
Tapping	·86	·92
Aiming	·70	·82
Match Stick Insertion	·56	·88

From Table R_1 it will be seen that the final 'reliability' of the motor tests given to Group D is greater than the initial 'reliability,' and that the final 'reliability' is, in every case, very high. It should be borne in mind that this final 'reliability' refers to performances of the tests whose intercorrelations are as negligible, at least, as those of their earliest performances.

Table R_2.

Correlation coefficients of each two successive performances of the same tests (in the case of Group D) from the first to the ninth performance.

	Performances correlated							
Tests	1 and 2	2 and 3	3 and 4	4 and 5	5 and 6	6 and 7	7 and 8	8 and 9
Tapping	·66	·86	·88	·78	·82	·90	·78	·80
Aiming	·20	·55	·76	·74	·82	·76	·58	·78
Match Stick Insertion	·60	·64	·50	·56	·44	·48	·66	·62

The coefficients shown in Tables R_1 and R_2 are high enough to make it clear that the negligible intercorrelations of the motor tests given to Group D, at least, were not due to the unreliability of the individual tests.

(4) It was pointed out in Section I that the present investigations were undertaken with a vocational motive. The original problem was: Can we find vocational tests for types of industrial work where the chief demands involve motor activities? The results obtained indicate pretty clearly that for every single kind of such industrial work a special test (or tests) would be required. For instance, picking up and packing chocolates will be found not to correlate with label pasting: a test that will detect the capacity required for the former will not detect the capacity required for the latter. And so generally. It is clear that there can be no general motor test for detecting persons of 'motor type,' because there is no motor type. Motor capacities appear to vary independently of one another. The individual who seems to be of 'motor type,' in that he excels in each of a number of motor activities, would be, on this view, the person who happens to possess a high degree of each of the motor capacities required for the performance of these activities; but an individual's performance in one such activity is not in general the slightest indication of what his performance in another such activity will be.

V. CONCLUSIONS.

(1) There is no 'motor type.' Terms such as 'motor dexterity,' or 'practical ability,' are misleading since a person's relative performance in any one motor test does not normally represent a general motor test performance level, that is, a relative performance level which will be attained in any other motor test.

(2) Motor capacities are relatively independent of intelligence. For since motor tests do not correlate with one another positively to any appreciable degree, they cannot in general correlate positively with any other tests, and therefore not with intelligence tests.

(3) From the point of view of vocational guidance, the practical conclusion is that every occupation which consists mainly of a routine performance of specific movements will require specific vocational tests; that is, motor (vocational) tests for a given 'motor' occupation must be tests of just those motor capacities that function in that occupation.

(*Manuscript received* 2 *March* 1922.)

A VINDICATION OF THE RESONANCE HYPOTHESIS OF AUDITION. V.

BY H. HARTRIDGE.

(*From the Cambridge Physiological Laboratory.*)

1. *Introductory* (p. 185).
2. *The inverse proportion between 'sharpness of tuning' and 'persistence' of resonators of different pitch, according to physical considerations* (p. 186).
3. *The inverse proportion between the 'sharpness of tuning' and 'persistence' in the ear, according to auditory experiment* (pp. 187–190).
4. *Obedience to Weber's law* (pp. 190–193).
5. *Principal experimental evidence in favour of the resonance theory* (pp. 193, 194).
6. *Summary* (p. 194).

1.

IN discussing the relationship between the persistence and the sharpness of tuning of resonators, Helmholtz describes tests which show that shakes and trills which appear quite sharp and musical when executed at a certain rate in the treble, appear bad and rough when performed at the same rate in the bass. He points out that these facts prove that "there must be different parts of the ear that are set in vibration by tones of different pitch and which receive the sensation of these tones," or in other words that the ear contains a series of resonators.

I find on the lines of this suggestion that it is possible to test by experiment whether there are resonators in the ear or not. The test is based on the following facts:

(*a*) That the tuning coefficients and persistence coefficients of resonators of different pitch are inversely proportional to one another.

(*b*) That the tuning and persistence coefficients of the ear for the perception of notes of different pitch are inversely proportional, and therefore the ear must contain resonators.

2.

The inverse relation between the tuning and the persistence coefficients of resonators can be readily seen by an inspection of Helmholtz's first table which is reproduced in a modified form herewith. In column A is given the 'tuning' coefficient, *i.e.* the percentage difference of pitch necessary to reduce the intensity of sympathetic vibration of any resonator to one-tenth the value it would have if the incident sound waves were exactly in tune with that resonator.

In column B is given the 'persistence,' *i.e.* the number of vibrations the resonator will execute between the moment at which the sound waves that set it in motion have ceased, and the moment when its rapidly dying vibrations have fallen to one-tenth the intensity that they possessed during the time the sound waves were arriving.

An inspection of Table I will show that the 'persistence' and the 'tuning' coefficients are inversely proportional.

Table I.

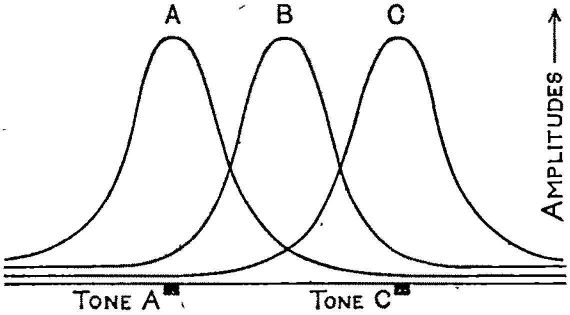
Tuning	Persistence
1·5 % difference of pitch	40 vibrations
3 " "	20 "
6 " "	10 "
12 " "	5 "
24 " "	2·5 "

It should be pointed out that this inverse proportionality does not depend on the particular units used to express persistence or tuning. Thus the difference in tuning may be expressed in tones or in decimals of an octave, and the statement still holds true. Further, instead of the intensity of vibration being reduced by definition to one-tenth, any other fraction could be assumed instead, without affecting the inverse proportionality between the coefficients of damping and tuning.

It should be noted further that no special form of resonator is referred to, as Helmholtz states "the connection between the two relations (of persistence and tuning) can be calculated independently of the nature of the body put into sympathetic vibration." It is as true of an electrical circuit as of a stretched string. Any series of resonators, no matter what their type, will then show the inverse relationship between damping and tuning. But further, if the necessary conditions are obeyed, then it is almost certain that no mechanism will show the above relationship unless it contains resonators; the necessary conditions being that the inverse proportion be tested over a large enough range of tones.

3.

Mayer[1] measured, for his own ear, the differences in pitch required in order that the beats produced between two tones should just disappear. While physically any two tones whatever should produce beats of some period when sounded simultaneously, physiologically this is not the case, for a sensation of beats is only obtained within a certain range of beats per second, a range which is found to depend on the frequencies of the notes generating the beats. Helmholtz[2] calculated the physiological intensity of beats from the tuning coefficients of the ear resonators in the following manner. Supposing the ear resonators to have the tuning coefficients for different amplitudes shown graphically by one of the curves in Fig. 1. Then no matter what may be the pitches of two tones simultaneously sounded, the resonators of the ear set into vibration by them can be divided into three groups. Group A containing those set into motion by tone A alone, group C those set into motion by tone C alone, and group B those set into motion by both tone A and tone C. Now neither group A nor group C will show any beating effects but only group B because for beats to be effective both tone A and tone C must operate together to set the resonator into vibration. Regarding these curves *A*, *B* and *C* in Fig. 1 to represent typical members of each of these groups, it is a simple matter, as Helmholtz showed, to calculate the intensities of vibration of A, B and C and thus to determine the relative intensity of B to those of A and C or in other words to ascertain the relative intensity of the beating effect to the intensities of the tones.



Frequencies.

Fig. 1.

Thus suppose that the intensities of *A* and *C* in the diagram be both equal to 100 (represented by the heights of *A* and *C* above the base line), while the intensities of *B* is equal to 10 from each tone by itself and therefore to 20 from both together; then the beating intensity will vary between a maximum of 40 when the crests and troughs of the sound waves of the two tones undergo summation, and zero when they interfere. The whole intensity of sound due to the two notes will therefore be on the average 100 due to each tone, and 40 for the beating effect, that is a total of 240, and this will fluctuate between 240 and 200 that is by approximately 20 per cent. If now we consider in a precisely similar manner the case where the two tones are further apart than in the above case, it will be found that the beating effect will be less, because both tones being less in tune with the beating group of resonators, the vibrations of the latter will have a smaller amplitude; *vice versâ*, if the tones are closer together the beating effect will be greater. Now, if instead of changing the tones the properties of the resonators could be changed without altering their natural period, then it is readily seen that making the tuning sharper would reduce the beating effect, and *vice versâ*. If then we assume that the same percentage of beating effect affects all the parts of the organ of Corti similarly, so that if beats of say 1 per cent. intensity are just perceived by the hair cells in one part of the cochlea they are also just perceived by all the others, then we can ascertain whether the tuning of all the resonators is equally sharp by ascertaining whether the same difference of tone just causes beats to cease at different parts of the musical scale. If, on the other hand, one part is found to require a smaller tone difference than another, then so much the sharper must be the tuning of the corresponding resonators. The following values calculated from data obtained experimentally by Mayer[1] show that the tuning increases in sharpness as the natural period of the ear resonators rises.—

---

[1] *Amer. J. of Sci.* Jan. 1894, XLVII.

[2] *Sensations of Tone*, Ellis's translation, 4th ed. 1912, 172.

Table II.

Mean tone	Percentage difference of tone required to stop disonance	Tuning coefficient
128	12·70	1·27
192	10·93	1·092
256	10·00	1·0
320	9·45	·945
384	9·07	·907
512	8·45	·845
640	8·15	·815
760	7·82	·782
1024	7·22	·722

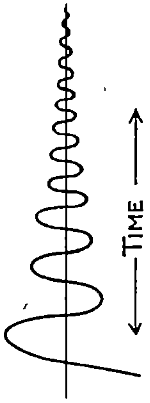
N.B. The figures in the last column are calculated from those in the middle column by making coefficient for tone of 256 = 1 and reducing all others in proportion.

---

[1] *Loc. cit.* 18.

The above figures show that the resonators responding to the tone 1024 vibrations per second have 76 per cent. sharper tuning than those corresponding to a pitch of 128 vibrations per second.

Calculation shows that the intensity of vibration falls off very rapidly at first, but with increasing slowness after the incoming wave motion has ceased to actuate a resonator, so that although in one vibration its amplitude may be reduced by one-half yet a very large number of vibrations indeed would have to occur before it could be said to have come finally to rest. This rule is illustrated diagrammatically in Fig. 2. Helmholtz defined the persistence coefficient used in his tables as the number of vibrations which would occur before the intensity of vibration was reduced to one-tenth that set up previously by the wave motion.



Amplitudes.
Fig. 2.

Now it is clearly not possible to measure the persistence coefficient in the case of the ear resonators because we have no means of estimating the number of vibrations before the amplitude falls to one-tenth of its previous value. We can, however, measure the duration in seconds of an interruption in a musical tone which just passes unnoticed, and from this we can infer any difference in the damping coefficients for different resonators in the following manner. Assuming as in the previous section that changes in intensity of sound of say 1 per cent. are just perceived by the hair cells in all parts of the cochlea, then if we measure the longest interruption in a musical tone which just passes unnoticed, we shall have ascertained the length of time during which the intensity of vibration of the resonators concerned has just not fallen by 1 per cent. Knowing this, we can calculate the number of vibrations that would occur in such an interval of time, that is the number of vibrations that must occur before the intensity of vibration falls by 1 per cent. If then this number is the same for all tones, then the damping powers of all the resonators must be similar; whereas if the number of vibrations increases then the damping powers have decreased and *vice versâ*.

The following figures calculated from data obtained experimentally by Mayer[1] show that the damping power decreases as the natural period of the resonator rises in pitch.—

---

[1] *Loc. cit.* 18.

Table III.

Tone	Time in seconds	Number of vibrations	Persistence
128	·0139	1·78	·861
256	·00801	2·062	1·000
320	·00685	2·19	1·075
384	·00568	2·18	1·060
512	·00463	2·365	1·147
640	·00397	2·54	1·230
768	·00350	2 68	1·299
1024	·00294	3 01	1·456

The 'persistence' given in the last column is calculated as in Table I, on the basis that the resonator for tone 256 is the standard with coefficient = 1, all the others being in proportion.

The above figures show that the resonators responding to 128 vibrations per second have 70 per cent. more damping than those responding to the pitch of 1024 vibrations per second.

Having thus obtained the tuning and damping coefficients of ear resonators of different pitch we are now in a position to test whether the one is the reciprocal of the other as Helmholtz's calculations make necessary if the ear does contain resonators.

Table IV.

Tone	Tuning coefficient	Reciprocal of tuning coefficient	Persistence coefficient
128	1·270	·7874	·861
256	1·000	1·000	1 000
320	·945	1·058	1·075
380	·907	1·103	1·060
512	·845	1·183	1·147
640	·815	1·227	1·230
760	·782	1·279	1·299
1024	·722	1·385	1·456

Inspection of the figures in the last two columns of the above table will show that there is quite close agreement between them (the average error being 3 per cent.), and that therefore the damping and tuning coefficients of different resonators are very nearly inversely proportional, that is, that the ear resonators behave almost exactly as Helmholtz calculated from first principles that they should. Therefore it is almost impossible that the correspondence between experiment and calculation should be as close as the above tables show, unless indeed there are in fact resonators in the cochlea.

---

[1] *Loc. cit.* 14.

4.

It will be recollected that both the tuning and damping coefficients were measured on the assumption that the hair cells perceive the same percentage difference of sound intensity, say 1 per cent. Now although in the case of the retina the foveal cones are found to respond fairly consistently to a certain percentage difference of intensity of light, and therefore presumably the ear could react to sound in the same way, yet the final conclusion arrived at, namely, that damping and tuning are inversely proportional, is independent of any such assumption, as may be shown as follows. Suppose that while one set of hair cells belonging to a particular resonator responded to a difference of 2 per cent. in the sound intensity, all others responded to 1 per cent. Then the tuning of this resonator, other things being equal, would appear 1·44 times as sharp as that of a resonator responding to 1 per cent. But the damping of the 2 per cent. response resonator will appear $\frac{1}{1{\cdot}44}$ times as good as that of one responding to 1 per cent., since a larger number of vibrations will have to occur before there is an appreciable fall in the intensity of vibration. When therefore the tuning and persistence coefficients are considered together, it will be seen that the one has increased by as much as the other has decreased and therefore the one coefficient is still inversely proportional to the other. It is therefore not necessary for the ear resonators to respond to the same difference threshold of intensity in order that they should conform to the requirements of the resonance theory.

Since the two values obtained by experiment are both independent of the actual difference threshold of intensity of sound, we cannot use them in order to obtain any evidence for or against obedience to Weber's law.

Mayer gives other experimental evidence, however, from which the conclusion may be drawn that the ear very nearly obeys this law. "Great differences in the intensities of the pulses had little effect on the number of beats required to produce a continuous sensation." That is, a loud and a soft tone both required approximately an interruption of the same shortness for it to pass unobserved. Since the dying vibrations of a resonator are each a constant fraction of the initial amplitude, the resonators after loud, medium or soft tones are reduced in intensity by the same percentage amount in unit time. And therefore from the above experiment the ear always detects a certain percentage decrease, no matter what the loudness of the tone may have been; that is Weber's law is obeyed to a close approximation.

We have seen further in Tables II and III that for the tones of 128 and 1024 vibrations per second which are in the ratios of 1 and 8, *i.e.* are three octaves, apart, the persistence and tuning coefficients change in the ratio of 1 and 1·725. We have also seen that doubling the value of the difference threshold of sound intensity of the ear changes the persistence and tuning coefficients in the ratio of 1 and 1·44. We could therefore explain the change in the persistence and tuning coefficients found by experiment on a physical basis, *i.e.* by a change in the degree of damping of the resonators, or on a physiological basis, *i.e.* by a change in the difference threshold. With regard to the latter, in order to account for a ratio of 1 and 1·725 we should have to change the difference threshold by 2·4 times. But if this were the amount required for three octaves, that required for the eight or nine octaves of the audible scale would be difference thresholds for the two ends of the scale in the ratio of 1 and 8 with one another. Now we know that, for example, the difference thresholds for pressure on the skin vary from part to part by nearly as much as 1 and 8. So that the above physiological explanation is quite feasible. With regard to the physical explanation, there is against it the fact, pointed out by Helmholtz[1] that high-toned reeds and strings yield tones which die off very rapidly, *i.e.* they are more highly damped than are low-pitched ones, whereas with the ear resonators the opposite is found to be the case. This criticism may in its turn be criticized because although high tones die off more rapidly, it does not mean that a larger number of vibrations may not have been performed in the length of time during which the tone continued to sound. Thus in Table II we see that the length of time of a just-unnoticed interruption is roughly five times as long for a tone of 128 vibrations per second as it is for a tone of 1024 vibrations per second and therefore if the resonators corresponding to both these tones could be set in motion mechanically by striking or plucking, then the lower tone would last five times as long as the higher, and this in spite of the fact that as we have seen it is the lower that is the more strongly damped than the upper. The same conditions may quite easily apply to a stretched string or a reed.

In the second place, the greater damping of the lower tone resonators would find a ready explanation in Wilkinson's view[2] that since the lower tone resonators are found in the upper whorls of the cochlea, a greater mass of fluid must be set in motion before movements of the stapes piston can be imparted to them, than is the case with the shorter and nearer resonators of higher pitch. Since a larger mass of fluid has to be kept in motion for the lower tone resonators, the greater is the fluid friction, *i.e.* the larger is the damping of that motion. Now if the eight or nine octaves representing the range of audible tones (15,000 to 50 vibrations per second) be spaced equally say with 2 mm. for each octave, the tones of 1024 and 128 are found to occur roughly 38 and 14 mm. distance respectively from the piston of the stapes. If then the fluid friction is proportional to this distance we should expect the degree of damping to vary as these distances, *i.e.* as 8 is to 14 or as 1 is to 1·75. But we have seen above that the persistence and tuning coefficients do actually vary by 1 to 1·725. The above explanation would therefore appear to be a perfectly feasible one.

---

[1] *Loc. cit.* 172.

[2] *J. of Laryngol. and Otol.* Dec. 1921, 557.

We can summarise the above by saying that both physical and physiological explanations for the differences in tuning and persistence shown in Tables I and II are possible, and that at present we cannot judge between them. It is therefore only possible for us to say that for some cause at present undecided the 'effective' tuning and persistence of the resonators of different pitch vary. Fortunately this knowledge is sufficient for practical purposes. For example, it matters very little to us that a particular resonator is still in vibration five seconds after the tone setting it into motion has ceased, so long as the hair cells attached to that resonator are not stimulated thereby.

5.

The principal experimental evidence in favour of the resonance theory is:

(1) That with two pure tones of different pitch the response of the ear is independent of their relative phases.

(2) That the sudden change in the phase of a tone by π is found to cause a temporary interruption of the sensation.

(3) That pathological changes in limited parts of the organ of Corti are produced by prolonged stimulation by pure tones. Lower parts with short resonators being found affected by high tones, higher parts with long resonators by low tones.

(4) That fatigue to one tone affects the response of the ear to that tone only, and not to other tones.

(5) That the tuning and persistence coefficients of the ear for different tones are found by experiment to vary inversely with one another, as

physical considerations show that they ought to do if the ear contains resonators.

6.

The contents of this paper may be summarised as follows:

(1) Physical considerations show that the coefficients for sharpness of tuning and persistence should be inversely proportional to one another for resonators responding to tones of different pitch.

(2) The coefficient for sharpness of tuning may be calculated from experiments by Mayer. The tuning coefficient is found to decrease approximately by one-half as the pitch rises by ten times.

The coefficient for persistence may also be calculated from other experiments by Mayer. This coefficient is found to increase by approximately twice for a rise of pitch of ten times. When the tuning and persistence coefficients of the ear for tones of different pitch are compared, they are found to vary very nearly in inverse proportion with one another. It is very unlikely that this relationship would be found unless there are resonators in the cochlea.

(3) Experimental evidence is available which shows that the ear shows approximate obedience to Weber's law.

(4) The main experimental evidence is recapitulated on which the resonance theory is based.

(*Manuscript received* 31 *December*, 1921.)

SUSPICION[1].

BY ALEXANDER F. SHAND.

I. *The causes and social effects of suspicion* (pp. 195–197).
II. *Analysis of the attitude of mind of suspicion* (pp. 197–199).
III. *Of the instinctive behaviour of suspicion* (pp. 200–202).
IV. *Of two kinds of strength which suspicions have and two meanings of the term 'suspicion'* (pp. 202, 203).
V. *Of the emotional constitution of suspicion* (pp. 203, 204).
VI. *Of the derivation of suspicion* (pp. 204–206).
VII. *Of the influence of repugnance in suspicion* (pp. 206–210).
VIII. *Of the ends of suspicion and the significance of the complex and secondary emotions* (pp. 211–214).

I. THE CAUSES AND SOCIAL EFFECTS OF SUSPICION.

IN times of war and social disorder certain emotions become prominent which at other times are little noticed. One of these is Suspicion. Disagreeable in feeling, it is opposite to the pleasant emotion of Confidence or Trust. Yet it is as frequently displayed by some persons as Trust by others. There is a temper of Suspicion. Brave and generous natures are not prone to it, nor are the happy: "celui qui est dans le bonheur, ne forme aucun soupçon[2]." But those who have been deceived and plundered, those who have formed a bad opinion of human nature, acquire it. The temper of Suspicion is also exhibited in some forms of mental disease: in delusions of persecution the patient is prone to suspect everyone.

Besides individual, there are social forms of Suspicion where the emotion spreads from one person to others in a social group or class, or in the nation at large, by suggestion, in response to prevalent social conditions. In times of civil war and revolution men suspect their neighbours and even their friends. In the great war men watched suspiciously the aliens from enemy countries in their midst, and refrained from intercourse with them. In contrast to these conditions, when there is peace and the laws are just and give adequate protection to life and property, men have confidence in one another, and travel without suspecting that those whom they meet may be prepared to do them injury.

---

[1] Written in 1916 during the Great War.

[2] Mme d'Epinay, quoted in *Dictionnaire Larousse*, Art. 'Soupçon.'

From the causes of Suspicion let us pass to consider its effects. If we have become suspicious of anyone in business we refrain, as far as we can, from employing him in future. If we are suspicious of the board of some company we refrain from investing in its shares. If we are suspicious of someone we have met at social gatherings we refrain from inviting him to our homes. In the great war enemy aliens were set apart in places under guard so that they could not communicate and have dealings with those outside. Thus (1) *Suspicion tends to destroy social intercourse, so far as we act upon it; and the wider it spreads the more it paralyses the life of the community which reposes on some degree of confidence between its members.*

The unscrupulous politician slanders one class of the community to another. His sweeping accusations may be little capable of proof, but they spread by suggestion and infect the class-mind. This social form of Suspicion may not have its full effect in putting an end to all co-operation between the classes concerned, since each of these depends for its livelihood on the other, yet its effect is still manifested in a lesser degree. Men filled with suspicion of employers and 'capitalists' will put no good will into their work, which tends to become slack and inefficient. Hence the business community cry out that without the restoration of 'confidence' between classes there can be no general recovery of trade; that through the spread of 'suspicion' strikes become more frequent, unemployment greater, and the costs of commodities increased.

When we reflect on the ease with which suspicions are engendered and the difficulty with which they are eradicated, that monstrous charges to be fully believed in must seem to be supported by evidence, but to arouse suspicion need not be, that suspicions once formed tend to become permanent attitudes of mind toward the person or the class suspected, that such permanent suspicions are the common source of jealousy and sentiments of hatred, that a temper of suspicion once fixed vitiates character by disposing it to hatred instead of love—we shall be astonished that an emotion that has such great social importance has been ignored by psychology as if it had none; and that the law which renders a man who defames the character of another liable to action for slander or libel, when he publicly and contrary to the general harmony and welfare defames the character of a whole class renders him liable to no action whatever, though the pernicious effect of his action is on a scale so much greater.

While Suspicion has not been studied by psychologists, and seldom even mentioned by them[1], literary men have occasionally noticed it. Bacon in his *Essays* has one upon 'Suspicion.' As statesman and observer he realised its importance, and summarises in his succinct way its evil effects. "Suspicions," he says, "are to be repressed or well-guarded; for they cloud the mind, they lose friends, and they check with business, whereby business cannot go on currently and constantly; they dispose kings to tyranny, husbands to jealousy, wise men to irresolution and melancholy...[2]."

Without attempting at first to trace to its source an emotion the source and composition of which are certainly obscure, we shall try to form a clear conception of it as developed in ourselves by distinguishing (1) its thoughts and attitude of mind, (2) its tendencies and behaviour, and (3) its feelings.

II. ANALYSIS OF THE ATTITUDE OF MIND OF SUSPICION.

(1) The attitude of mind of Suspicion contains doubt, and its thoughts (when it has them) are doubtful thoughts, and refer, however obscurely, to the character of the suspected person. We doubt whether he has some design to do us injury, whether he is honest, or not rather dishonest. For these doubts are not equally balanced between the opposite alternatives, but incline always to adopt the worse rather than the better conception of his character. We do not say we 'suspect' a man of being 'honest,' but of being 'dishonest.' There is something in the nature of this emotion that inclines us to imagine evil without good cause, and sustains the evil thought as fear sustains the thought of danger. It is here that 'suspicion' is distinguished from 'distrust,' which if not impartial is not prejudiced in the same degree. What protects us against unjust suspicions is the opposite emotion of 'confidence'; yet suspicion may be insinuated into loving and trustful natures with disastrous results, as we see in the case of Othello. Wherefore Bacon concludes that suspicions must be "repressed, or at least well guarded," and especially those that are "put into men's heads by the tales and whisperings of others"; for these "have stings[3]."

These fluctuating doubts of Suspicion appear to be essential conditions of the emotion. For if we had them not, but, in their place, the requisite knowledge, our suspicion would be at an end. There is a second condition. The suspected person is felt to have some power for mischief. We may have doubt about a person with an inclination to put a bad construction on his actions; but if he is a long way off he may not be able to harm us. Suspicion implies that he is near enough to do mischief to us or to someone we love; for an ill-disposition then gives anyone some such power.

---

[1] Since this article was written Dr Ch. Mercier has published a work entitled *Our Temperaments* in which one of these is assigned to Suspicion.

[2] Essays, 'Of Suspicion.'

[3] *Op. cit.*

(2) Let us consider next the tendencies and behaviour of Suspicion. Although an unpleasant emotion it is attentive to and watchful of its object. Hence Shakespeare says:

> Suspicions all our lives shall be stuck full of eyes;
> For treason is but trussed like a fox,
> Who ne'er so tame, so cherished and locked up,
> Will have a wild trick of his ancestors.

A second tendency of Suspicion is secrecy in action. Its watchfulness has not the candour of common observation. It watches secretly to put the suspected person off his guard; since, if his intentions are hostile, he will have the more reason for concealing them. Hence Swift speaks of the suspicious man as "often turning and slinking through narrow lanes[1]," and Bacon observes that "suspicions among thoughts are like bats among birds, they ever fly by twylight[2]."

These two tendencies of watchfulness and secrecy are combined in Suspicion, and both suggest that, as the emotion starts from doubt and ignorance, its end is knowledge—knowledge of the man's intention toward ourselves or others. Yet this does not seem to express its full significance, which is rather to prepare the way for some practical activity to which this knowledge is a means.

There is a third tendency in Suspicion which is central and gives a clue to its end. This tendency operates at once when we suspect a man may attack us or those we love—and this must be the primitive form of the emotion. Here the situation is uncertain and dangerous. To remain passive in face of it will increase the danger. It may change for the worse at any moment. Therefore we must be alert and prepared for whatever may happen. Watchfulness and secrecy are a part of this preparedness, but not the whole of it. The body as well as the mind must be alert and ready to act. For the end is not knowledge merely, but preparedness for action.

What is the action? The doubt in Suspicion implies that we do not know. If we knew how the suspected person was going to act, and what his intentions were, we should 'suspect' no longer. The patrols of an army are thrown out in front and on the flanks, and watch the movements of everyone with suspicion, moving stealthily, sending back information of what they discover, so that the army behind may be better able to estimate the power and intentions of the enemy, and be prepared for attack, for defence or retreat according to their knowledge of the situation. This preparedness seems to be the end of Suspicion; and it implies a readiness to adopt at the moment any one of several alternative kinds of action after we have acquired a fuller knowledge of the situation.

---

[1] Quoted in Johnson's *Dictionary*, Art. 'Suspicion.'

[2] *Op. cit.*

This state of general preparedness seems to be all that Suspicion accomplishes. Herein lies its utility. Without it we should be liable to be taken unawares, overwhelmed by surprise, and thrown into confusion; and before we were able to recover from it, the opportunity might be lost, and the requisite action come too late. The hurried preparations of the British nation, after war with Germany was declared, under the incitement of fear and anger, according to human calculations, should have come too late, and were very different in amount and cost from what could have been accomplished in the preceding years of peace, had its politicians foreseen what was probably and plainly approaching, and roused the Nation to a well-grounded suspicion instead of lulling it into a foolish confidence.

We must therefore include in the behaviour of Suspicion not merely the primitive and relatively simple actions that make an animal start to its feet and stiffen its muscles when it suspects anything, and watch closely the approach of an intruder to know whether he be friend or foe, but also the elaborate and prolonged preparations which nations make for their security in face of one another so as not to be overtaken by surprise. Whatever excesses may be attributed to Suspicion (as to all other emotions) its biological value seems therefore to be incontestable.

Let us tentatively express this law of Suspicion: (2) *Suspicion tends to prevent our being taken by surprise on the approach of danger by rendering us prepared in advance to adopt at the right moment, the right action.*

We must not confuse with the behaviour proper to Suspicion those actions which ensue upon it when doubt has passed and we know what we have to do. As a result of its timely intervention other emotional and instinctive tendencies replace it. The animal takes to flight, or attacks its enemy, or finds that the suspected intruder is a friend. Some instinct of fear, anger or of kindly emotion has replaced Suspicion.

III. OF THE INSTINCTIVE BEHAVIOUR OF SUSPICION.

It would seem to be of biological importance for animals as well as human beings to inherit a disposition to Suspicion. An instinct of Suspicion, if such there were, would render an animal acutely sensitive to any event in its neighbourhood which might be a sign of approaching danger, and would enable it to respond as quickly as possible to this stimulus by adopting an alert and watchful attitude. Such an instinct would be comparatively simple in respect of its behaviour, yet clearly above the level of reflex action. The consciousness and intelligence of the animal is brought into play and influences the action. The animal does not merely rouse itself from lethargy and start to its feet, but it waits, it watches or it listens. If it ran away at once as some animals may do, there would be no suspicion. If it attacked at once as others may do, there would be no suspicion. If it had no doubt, there would be no suspicion. But it awaits in doubt, concentrated in anticipation of some event that will determine its future action. This instinctive behaviour may vary from one species to another, where one depends more on sight, another on hearing, another on smell.

Innate sensitiveness to signs of possible dangers and instinctive adaptations to meet them many species of animals possess. We may, however, think that this does not require us to posit a new instinct. In fact, so far as there is evidence of any instinct at the base of Suspicion, we may be inclined to infer that it is one of fear; for Suspicion bears a certain resemblance to fear: or one of anger; for Suspicion has also some resemblance to anger. If we assume that the instinct at the base of Suspicion is only one of fear, it is committed beforehand to react according to fear. If, on the other hand, we assume that the instinct at its base is only one of anger, it is committed beforehand to react according to anger. The significance of the state of doubt in which Suspicion subsists is that it is committed to neither, though prepared for both. Still other alternatives are possible. When the state of doubt is resolved we may find that our suspicion was unfounded. In other cases we cannot resolve this doubt, and the suspicion continues. The tendency of this kind of Suspicion, we have already seen, is as far as possible to put an end to social intercourse. For the sight of a man whom we suspect is repugnant to us and we tend to avoid his company.

Now it is true that ordinary fear, where it does not take us by surprise, or paralyse us by its intensity, involves some degree of preparation. It rouses us from lethargy and puts us on our guard. Under the influence of fear a man may consider what course is best for safety, and prepare himself to adopt this course. But the more primitive and unrestrained fears have not this reflectiveness. An animal follows the instinct of fear which the situation evokes. It takes to flight, or conceals itself, or remains motionless, as the case may be. The selection is made at once. There may be a moment's uncertainty—as in which direction to flee; but it is quickly resolved. Under the influence of anger there may be also a certain degree of preparation, as in getting the body into a posture for attack. In both cases the preparation seems very different in degree and kind from the preparedness of Suspicion. The one is particular, the other general; in the second the state of doubt or uncertainty is usually more prolonged. In observing animal behaviour, however, it is difficult in many cases to decide whether an animal is under the influence of suspicion or of fear or anger. The little birds as they peck at small objects on the ground, are ever prepared, watching and listening; and after each peck or two, raise their heads, and look to right and left before proceeding to take their next morsel. Their situation, when pecking on the ground, exposes them to attack, and is therefore innately connected with an impulse to raise their heads again as soon as possible, and to look around. Are they under the influence of suspicion or of fear, or does the instinctive action, so rhythmical, so constantly repeated, take place without any emotion? We should answer, in accordance with our rule, that there is probably no emotion, as long as the instinctive behaviour responds to the stimulus without obstruction. Whether, in other cases, it be suspicion or fear may depend on whether the doubt present before the bird looked round provided for the possibility of different responses according to the nature of the object perceived—as whether a cat was approaching or only another small bird, or nothing at all—so that in the one case it would fly away, in the others remain undisturbed or show hostility. And here we are not able to judge by the expression of the eyes as in the case of human beings, in whom suspicion, when not artfully concealed, is so easily recognised.

It is therefore difficult to decide from the observation of most animals whether suspicion is present or not, or whether even they are capable of feeling the emotion. The slow-moving cattle, when we approach them in a field, where they are lying down and leisurely chewing the cud, rouse themselves, if we come too near, from their lethargy, and rise to their feet, gazing at us the while in their dull way. When we come still nearer, though without showing hostility, they seem to hesitate for a few moments as if undecided what to do, before they sheer off and retreat.

If we have a dog with us they may hesitate no longer but, notwithstanding our presence, offer to attack it. In both the above cases the suspicion, if suspicion it be, seems unlike ours, being without its secretiveness—or the trying not to disclose its watchfulness. In the little birds their looking from side to side may be an instinctive precaution of their fear; in the dull-minded cattle, an inability to appreciate at once what is happening, so that they are neither decided to react with their horns aggressively nor to retreat, but having risen to their feet, they observe us in a more general state of preparedness and doubt, which is more analogous to our human suspicions.

Let us now endeavour to see what light may be thrown on the nature of Suspicion by the analysis of the emotion in ourselves.

IV. Of Two Kinds of Strength which Suspicions have and Two Meanings of the Term 'Suspicion.'

There are two different kinds of strength which suspicions may have. The one lies chiefly in the strength of its conviction, the other in the emotional force accompanying it. Often where suspicions are strong in the one sense they are weak in the other. We may suspect strongly and reasonably the ruler of a foreign state of aggressive designs upon his neighbours. But if the nation to which we belong is far removed from the possible scene of danger, we may do nothing beyond showing alertness and attention to the course of events. A citizen of Chile or Argentina may have suspected the German government of aggressive designs before the opening of the Great War, and yet have considered that his country was out of danger. Here the strength of the suspicion means the high degree of conviction entertained concerning the hostile intention of the suspected persons, and the little degree of doubt to set against it, although the emotion itself is weak and hardly to be distinguished. In the second case, the suspicion may be strong as an emotional force, although the conviction of evil intention on the part of the suspected person may be weak, and the doubt concerning his intention considerable. Thus a husband may suspect his wife, realise the power for mischief of the man he also suspects, and the danger of his situation; though without having in his own opinion any conclusive grounds for his suspicion. Whence we may provisionally adopt this law of Suspicion: (3) *Whatever the strength of Suspicion as regards the conviction of evil intent on the part of the person suspected, Suspicion tends to be emotionally strong, other things equal, in proportion as the evil anticipated concerns ourselves or those whom we love, and emotionally weak so far as it concerns other persons.*

These two kinds of strength are reflected in the two uses of the term Suspicion. In the one it is used in a more general sense to signify a judgment of probability referring to some evil in things or persons, as when we say of the weather, "I suspect it will rain to-day," or of a person, "I suspect he is weak." Here there may be more or less doubt present, but little more feeling than is involved in our judgments in general. In the second, it is used in a more restricted sense to signify the particular emotion of Suspicion, which Johnson defines as "To imagine with a degree of fear and jealousy what is not known[1]." With regard to this definition, while the connexion with fear is close and general, jealousy is confined to those cases where the person suspected threatens some-thing or person that we desire to possess exclusively, as the woman we love, our power, or reputation.

V. OF THE EMOTIONAL CONSTITUTION OF SUSPICION.

Is fear then one of the roots of Suspicion as it is of Jealousy[2]? Bacon observes that suspicions "take place in the stoutest natures," instancing Henry VII, and that in them they do "small hurt," being usually "not admitted but with examination whether they be likely or no[3]"; but that "in fearful natures they gain ground too fast[4]." Suspicions therefore are increased by proneness to fear, which also makes it more difficult to control them. But there are some things that all men fear, as the loss of what they love most:

> "Where love is great the littlest doubts are fears;
> Where little fears grow great, great love is there."

Therefore all men are subject to Suspicion.

If fear is one of the roots of Suspicion, anger is another. For the expression of Suspicion on men's faces is always disagreeable, and arouses dislike, because there is a latent hostility in it, and criminals often acquire it. Swift says, "A wise man will find us to be rogues by our faces; we have a suspicious, fearful, constrained countenance, often turning and slinking through narrow lanes[5]." But if Suspicion only expressed fear and not also hostility it would hardly arouse this general dislike.

Sir Charles Bell appears to have overlooked the hostile expression of Suspicion. He describes it as "characterised by earnest attention, with a certain timorous obliquity of the eyes[6]." Yet the poet Spenser's personification of it, which he quotes, clearly distinguishes this hostile side:

---

[1] *Dictionary*, Art. 'Suspect.'

[2] *The Foundations of Character*, Bk. II, Ch. V, 2.

[3] *Op. cit.* [4] *Ibid.*

[5] Johnson's *Dictionary*, Art. 'Suspicion.'

[6] *Anatomy and Philosophy of Expression*, Essay V.

> Under his eyebrows looking still askance,
> And ever as Dissemblance laught on him,
> Lowering on her with dangerous eye glance,
> Showing his nature in his countenance.
> His rolling eyes did never rest in place,
> But walked each where for fear of hid mischance,
> Holding a lattice still before his face,
> Through which he still did peep as forward he did pace.

It is remarkable that so great an observer as Darwin should have made only a passing allusion to Suspicion in his *Expression of the Emotions*, and then only to group it with a number of states, as jealousy, envy, avarice, revenge and others, of which he remarks, that "It is doubtful whether the greater number of the above complex states of mind are revealed by any fixed expression, sufficiently distinct to be described or delineated[1]"; although he admits that "many of them can be detected by the eye[2]." If Suspicion had no distinctive expression it would be difficult to account for the fact that it is both easily recognised, and has received a distinctive name in so many languages. It is the stealthiness of its glance and the sudden way in which it is withdrawn, lest it be detected—'peeping' as it were through a lattice—which, joined to the menace in it and to curiosity, seem chiefly to distinguish the expression of Suspicion. This expression may be somewhat different in different situations. A criminal who fears detection, when in presence of several persons, may turn his glance 'askance' from one to the other, producing the 'rolling' motion of the eyes to which Spenser refers.

We conclude that both fear and anger have influenced the expression of Suspicion, together with the impulse of curiosity. The presence of this last impulse—not the emotion of wonder—is in no way remarkable, except in its persistency, since it is apt to be present at times in all our emotions.

VI. OF THE DERIVATION OF SUSPICION.

We have now two questions to ask. First, can we account for the emotion of Suspicion on the supposition that the dispositions of fear, anger and curiosity are simultaneously excited whenever it is felt? Secondly, can we account for its behaviour on the supposition that some degree of preparedness in a particular form is common to these emotions, and that here the preparedness becomes general, because fear and anger mutually restrict one another, and only the behaviour common to both can find expression? It seems that we can account for Suspicion in this way. There is nothing strange in supposing that a new kind of feeling can be generated when two or more emotions are blended together. As regards its behaviour, the concealment-instinct of fear is also expressed in the stealthy glance and the secret watchfulness of suspicion; and these characters are compatible with anger, as we see in the case of the *felidae*. Moreover, as the doubt of suspicion may last a long time, and is frequently never resolved, curiosity may last with it, coming to an end, if at all, when we know what the situation is with which we have to deal, when Suspicion itself is at an end, being replaced by fear, anger or some other emotion.

---

[1] Ch. XI.

[2] *Ibid.*

Let us now notice what is unsatisfactory and doubtful in this explanation, because we shall find that the same defects will attach more or less to the attempt to account for the derivation of all secondary emotions. In the first place, on the affective side, there is a gulf that we cannot bridge between the emotional dispositions that we allege to be the conditioning agents and the emotion resulting from them. Suspicion is neither fear, nor anger, nor curiosity. It is a new emotion: not a summation of the conditions, but a unique differentiation from them, with the quality that distinguishes it all its own. Because of its uniqueness we can never prove that our derivation of it is valid; for we cannot reverse the process and produce the emotion synthetically. We can at most find a certain resemblance between the cause and the effect. Suspicion has a certain resemblance to fear, a less degree to anger, and some to curiosity. So far the evidence of its derivation from them is slight, and little more than surmise; and most so-called derivations of the emotions are nothing more than this. And from this point of view it may be as easy to furnish a derivation of a primary emotion from a secondary, or what is worse from a sentiment, as we see in the frequent derivation of anger from hate[1]; for we can still trace a resemblance between them.

If we are to advance beyond this stage it would seem as if we had no other resource than the study of the expression and behaviour of the emotions in question to guide us. Thus in the present case we find something of fear, anger and curiosity expressed in the eye-glance of Suspicion, and the concealment-instinct of fear in its behaviour. We reach what we think is a probable conclusion; but it is no more than this. Even assuming the facts alleged to be correct, we cannot conclusively infer that because an *instinct* of fear is present in Suspicion therefore the

---

[1] Descartes, *Les Passions de l'Âme*, 3me Partie, Art. 199.

emotion of fear must have had some effect in the differentiation of the emotion of Suspicion. For the tendency is independent of the emotion, because the instinct to which it belongs can be operative without necessarily exciting the emotion. When for instance we find a concealment-instinct present in the aggressiveness of the *felidae*, as shown in all the behaviour by which they stalk their prey up to the final spring in which for the first time they disclose their presence with an overwhelming shock of surprise, we have no valid ground for concluding that the emotion of fear was also present in their consciousness from the beginning. For the *felidae* usually attack animals much weaker than themselves from whom they have nothing to fear. Their concealment-instinct is employed only as a method, on which the success of the attack depends with its shock of surprise, not giving the animal time to prepare for defence or flight. Sportsmen employ a similar method of concealment, because they know that otherwise they will not succeed in bringing the birds within range of their guns. In concealing themselves they feel, not fear, but the excitement of the chase. Therefore the presence of an instinct of concealment in the behaviour of Suspicion is not evidence that the emotion of fear is also present.

If we cannot ground this inference on the behaviour of Suspicion, can we not at least ground it on the expression of Suspicion? Here we are on firmer ground. For while we recognise that the same behaviour may be sometimes due to one motive, sometimes to another, yet when fear is clearly expressed in the eyes, face and bearing, then we infer with confidence that the emotion, in normal cases, is present.

On this ground we shall infer that the emotion of fear is operative in Suspicion, and further that in its wavering thoughts, driven hither and thither by doubt and curiosity, the emotions of anger also play a part, and for the same reason.

VII. OF THE INFLUENCE OF REPUGNANCE IN SUSPICION.

We have still to notice the influence of another emotion. In the first part of this article we considered the social effects of Suspicion, which we found were to arrest social intercourse. The influence of fear and anger does not seem to account for this effect. It seems rather to be due to the action of Repugnance. When we have come to suspect anyone he becomes repugnant to us, and we tend on this account to avoid his presence and neighbourhood. This emotion of repugnancy can have nothing to do with that general preparedness which we have traced to the combined influence of fear and anger. Nor does it in any degree solve the doubt, which the curiosity of Suspicion tries to solve, because this doubt suspends the action of both fear and anger. Rather it stultifies the action of all these emotions. Hence it follows that when the tendency of repugnance (which is to avoid suspected persons) prevails it leaves our suspicions of them just where they were. These are the suspicions that we often never get rid of. Their emotional form may die out or nearly die out; but the conviction of probable ill-intention remains. Thus Suspicion passes from one of its two forms to the other, and has now the kind of strength which we distinguished as due to conviction.

How do we account for this strange departure from the behaviour of Suspicion that we have been investigating, and how is it possible? It is not possible where life is at stake and, to guard it, we have to keep the suspected person under observation. In one of the old Icelandic Sagas of the 10th century when Law was only beginning to be established, and a man had to rely on himself and his kin for protection to life and property, there is a description of a very persistent suspicion in a brave and stout nature. The hero after whom the Saga is named, "Grettir the Strong," on account of his many misdeeds and homicides, is outlawed; and though he remains in the country, he has to betake himself to a solitary part, and there to live secretly. This man who, confident in his own extraordinary strength and skill, feared no foe, had one weakness—he feared the dark, and on account of it could hardly live alone. A man from the forests, also an outlaw, came to him and asked to be taken in. Grettir said, "You men of the forests are untrustworthy. But it is ill to live alone; I have no choice. Only he shall be with me who is willing to work at whatever comes to hand." The man had been bribed to kill Grettir, and stayed with him "right into the winter, and watched Grettir closely, but it seemed no easy matter to attack him, for Grettir was suspicious and kept his weapons at hand night and day; when he was wake the man would not venture to approach him." One morning the man came in and went to see whether Grettir was asleep. "Grettir lay still and did not move. There was a short sword hanging above his head. Grim thought he would never have a better opportunity. He made a loud noise to see whether Grettir took any notice, but he did not. So Grim felt sure that he was asleep. He crept stealthily to the bed, reached to the sword, took it down and raised it to strike. Just at the moment when he raised it Grettir sprung up on to the floor, and, seizing the sword with one hand, Grim with the other, hurled him over so that he fell nearly senseless. 'This is how you have proved yourself with all your friendly seeming,' he said. Then he got the whole truth out of him and killed him....So the winter passed. The hardest thing of all to bear was his fear of the dark[1]."

The same thing happens with a second forest man who came to him for shelter. To this man Grettir says, "I cannot risk taking in any more forest men." "I think," Thorir replied, "you have reason to mistrust forest-men." You have perhaps heard me spoken of "as a man of blood and a disturber of peace, but never did you hear of such a monstrous deed of mine as that I betrayed my host." In the end he persuades Grettir to take him in, to whom the latter says, "I will risk it with you; but know of a surety that if I suspect you of any treachery it will be your death." So they lived two years together, and Grettir "had never lived so comfortably since he had become an outlaw. Nevertheless he was so wary that Thorir got no chance." Thorir thinks over what he can do "to take Grettir off his guard" and at length makes a plan. One night a gale springs up. Thorir runs down to their fishing boat and breaks it in pieces, and returns to Grettir and tells him the storm has broken it, and has scattered the nets far out into the water. Grettir tells him to get the nets back, adding, "It seems to me to be your doing that the boat is smashed." Thorir says he cannot swim, and taunts Grettir with lack of daring to get them himself. Grettir answers, "I could get the nets...but betray me not if I trust you." There was a point of land running out into the lake, and the water was deep up to the shore. Grettir threw off his clothes and laid aside his weapons, swam out and got the nets. "Just as he was about to land Thorir quickly seized his sword and drew it. He ran toward Grettir as he stepped on to the bank, and aimed a blow at him. Grettir threw himself down backwards into the water and sank like a stone....Grettir swam beneath the water, keeping close to the bank so that Thorir could not see him, and so reached the bay behind, where he landed without letting himself be seen. The first Thorir knew of it was when Grettir lifted him up over his head and dashed him down with such violence that the sword fell out of his hand. Grettir got possession of it and without speaking a word cut off his head[2]."

In this realistic account the various features that we have distinguished in Suspicion are brought into clear relief. (1) The great biological importance of Suspicion, especially to primitive people, who, having learnt by concealment, deceit and stratagem to overcome strong animals will, if they have not Suspicion, be overcome by the same methods themselves. (2) As regards the behaviour of Suspicion we see that Grettir on both occasions wants to avoid the company of the forest-men. This is the first tendency which his suspicion discloses. But it is overborne by his dislike to live alone, largely due to his fear of the dark. (3) As he will not put an end to social intercourse between them, but chooses to live in close contact, first with Grim and afterwards with Thorir, Suspicion discloses its second tendency of watchfulness and preparedness for possible attack. This is a readiness to adopt at the moment any one of a number of alternative actions, such as flight, concealment or attack. For where the odds are too great he will take to flight, as when he throws himself into the lake and swims away hiding at the bottom, to renew the attack later. Thorir also is alert and watchful, not because he suspects that Grettir may attack him, but because he knows he has to attack Grettir. (4) The secrecy of both men is also marked and persistent, due in the case of Grettir not to fear, but to suspicion, by which he conceals his watchfulness of the suspected man; and this he carries to the length of pretending to be fast asleep, and so completely deceiving Grim who thinks that now the opportunity to kill Grettir has arrived; due in the case of Thorir probably to fear of the hostile and treacherous action he has to accomplish. (5) In Grettir's simulation of sleep, in his secrecy and watchfulness we see the influence of the curiosity of Suspicion, trying to resolve its doubt, trying to find out whether this man, so serviceable and smooth of speech, is friend or foe, leading him on to the crucial action. Then at length when doubt is resolved and suspicion at an end, his silent anger and the swiftness of its revenge.

---

[1] *Grettir the Strong*, Ch. LV.

[2] *Op. cit.* Ch. LVI.

Thus is it constantly with primitive men. The one who suspects that he will be attacked must keep his suspicion in mind so as to be always on his guard; the one who intends to attack must strive to make his enemy forget his suspicion, so as to surprise him when off his guard.

This remarkable case brings before us the central problem of Suspicion, which appears to have incompatible aims. We can sum these up in our first two provisional laws of tendency. The first states that Suspicion tends to put a stop to social intercourse with the suspected person. The second states that Suspicion tends to adopt an attitude of general preparedness for all events, which involves keeping a watch on the suspected person. Now it is clear that Suspicion cannot carry out both these tendencies simultaneously, but must select one of them. In the case referred to, Grettir sets aside the first tendency—that of avoiding the company of Grim and Thorir. He therefore has to follow the second tendency—that of maintaining a constant watchfulness and preparedness for whatever may happen. Can we then conclude that the first is the fundamental tendency of Suspicion, and that it is only when, for one reason or another, we cannot act upon it, that we fall back on the second?

There may be different reasons in different situations why we cannot avoid intercourse with suspected persons. We may suspect a man of intending to rob us, and our property may be already in his control. He may be a trustee against whom we have no provable charge. If we avoid him altogether we abandon the care of our property. We therefore act on the second tendency, and keep a close scrutiny of his actions. Here our chief aim is to pass from the doubt of suspicion to the certainty of knowledge.

In other cases we can act on the first tendency, as where, though we suspect a man's character, we do not suspect him of having ill-intention as regards ourselves, our family or friends. We say to ourselves, "Here is a man I suspect, I had better have nothing to do with him." A householder may suspect a servant and refuse to engage her. Here suspicion remains because we do not resolve its doubt; and in many cases we cannot. The second tendency is hardly brought into operation, or only for a few moments during our interview with the suspected person. Afterwards, having abandoned all intercourse with her, it is superfluous.

As regards the source of this first tendency we have thought that it is often due to the influence of the emotion of Repugnance, because ordinarily we find intercourse with suspected persons repugnant to us. But sometimes we may think that the tendency to avoid intercourse with suspected persons is more probably due to fear. For we do not know what evils may overtake us if we enter into close relationship with untrustworthy persons.

It is also difficult to distinguish between the flight-tendency of fear—when it takes the form of a retirement without precipitation—and the tendency to avoid things repugnant to us: here we distinguish them by their feelings. And in the suspicion that we feel for persons that are known to cheat at cards, or who are in the habit of borrowing money without any intention to repay it, we may feel a repugnancy to their company without fear.

We have already included fear, anger and curiosity among the essential conditions of Suspicion. The question is now whether we should not also include repugnance. If it often comes after Suspicion as an effect of it, fear and anger are also among its frequent effects. But they also accompany and condition its development, and this seems to hold of repugnance.

VIII. OF THE ENDS OF SUSPICION AND THE SIGNIFICANCE OF THE COMPLEX AND SECONDARY EMOTIONS.

In treating of the primary emotions we have to enquire what their ends are, because without a knowledge of these we cannot understand their systems[1]. This knowledge we derive from a study of their behaviour and the conscious aims—when they acquire them—which they set before themselves. What then is the end of Suspicion? From what we have now learnt of its behaviour it appears to have no single end. But if it has a plurality of ends, these are in the main the same as those of the emotions from which it has proceeded. Why then do we possess this emotion of Suspicion? What is its significance, if—apart from its uniqueness of feeling, which by itself cannot have much importance—it has no originality, and adopts the ends and tendencies of the emotions from which it is derived? Would they not equally serve our biological needs, and later purposes, without it?

We should make a great mistake if on these grounds we regarded Suspicion as superfluous. Rather if we rightly understand them they indicate what this new emotion has accomplished. The significance of Suspicion lies precisely in the fact that in it these precedent and conditioning emotions are not independent agents, but welded into a single system. Once this result is accomplished the new emotion is possessed of a range of adaptability to which they severally can make no pretence. It has a possible choice between their divergent tendencies. And in this versatility lies its peculiar and indispensable advantages especially to primitive people. If we revert to the case of *Grettir the Strong* we shall realise this more clearly.

Let us suppose that there were no such fact as Suspicion, but in its place only the primary emotions that have conditioned it, how would Grettir then have acted? He would have acted from one or other of them; for if all were aroused together Suspicion would again be generated. If then at first he had felt only Repugnance at the sight of the forestmen, he would have avoided further intercourse with them. In this way he would have found out nothing about either Grim or Thorir. He would have run great risks by his ignorance of their motives, since both of them had been bribed to kill him. He would not have suspected that in excluding them from his house they would lay a snare for him outside.

If, secondly, he had felt only Fear at the sight of them, he would have had some choice of action between the various tendencies of this emotion. He might have decided to flee from his house to some place at a safe distance. He might have concealed himself somewhere in the neighbourhood. If a coward instead of the brave man he was, he might have thrown himself on the protection of the forest-men. But he would have found out nothing about their motives, and would hardly have saved his life. If, thirdly, he had felt only Curiosity, he would have proceeded in his investigations in too open a way, and again would have found out little. And as he would have acted unsuspiciously, and without taking precautions for his safety, he would have been killed. Finally, if he had felt only Anger, he would have fought with each man prematurely, without knowing whether he were friend or foe, whether he were acting alone or with the secret support of others; who at the first onset might have rushed upon him to his surprise and confusion.

---

[1] See *The Foundations of Character*, Bk II, Ch. II, p. 197.

What then is our conclusion? Acting from the impulse of any one of these emotions, Grettir would probably have been killed. At least he would not have had as good a chance of saving his life as from his prudent, persistent suspicion, which, through organising in him the tendencies of its many emotions made him ready to adapt himself to many changes of situation. Fear might have been a partial substitute, but in comparison with suspicion how few alternatives would it have provided him, and how precipitate would have been its judgment! Through his suspicion he does not act precipitately, but waits a long time. Through it he finds out that this man is an enemy, not a friend. He brings him to the crucial test; and at last when the traitor is unsuspicious of danger himself, surprises and overwhelms him by the suddenness of his attack. This successful result would not have been possible through his fearlessness and courage alone, but to these only in conjunction with the watchfulness, preparedness, and prolonged patience of his suspicion.

There is in this complex and versatile emotion a degree of patience and reflectiveness which is not possible to any one of the primary emotions. Poised in doubt, it is not rushed prematurely into action. Divided between contrary impulses, it is forced to reflect on the advantages of each. In its suspense it prepares itself to adopt any one of them at need.

All the primary emotions subserve in diverse ways the preservation of the individual and of the species. So is it also with Suspicion. The difference between it and them is that while it is adaptable to a variety of different situations they are each confined to one. But whichever tendency of its complex nature is, in the actual situation, best adapted to the biological end of survival takes precedence over its other tendencies: thus in one situation, to avoid the suspected person; in another, to watch until the situation becomes clearer, and according to what it is then seen to be, to fight or to escape; in another, to continue to watch and make preparations indefinitely—as when nations face one another and heap up armaments, doubtfully, secretly, hostilely, because of their persistent mutual suspicions.

So far we have only shown that Suspicion has no single end in the sense that, at the resolution of its doubt or earlier, the end of now one, now another of its component emotions is adopted, so that what we finally see is the predominance of repugnance, fear or anger. What then becomes of Suspicion? Several events may overtake it. It may be displaced or extinguished. It is displaced in all those cases considered in the first part of this article where suspicion results in a suspension of intercourse with the suspected person. Here the result is due to repugnance; but Suspicion, though displaced, is not at an end. We suspect the person concerned the same as before, because the doubt and problem of suspicion are unresolved. Its disposition remains therefore connected with the same object. But the emotion passes out of consciousness because we cease to think of the suspected person; for having severed all intercourse and relations with him, we think that he can no longer do us harm.

The second event which may happen to Suspicion is that, through its curiosity, the problem of its doubt may be resolved. In this case Suspicion is extinguished. One or other of its component emotions replaces it. We act from fear, anger or repugnance, but no longer from suspicion. Thus when a government long suspected suddenly launches the attack, we suspect its intentions no longer: we know them.

A third possibility remains. We may neither be able to remove ourselves from the neighbourhood of the suspected object through following out the impulse of repugnance, nor be able to resolve the doubt and evil surmise of suspicion through following out the impulse of its curiosity. In this case Suspicion is neither displaced nor extinguished. It continues active and evokes its characteristic behaviour, instead of as in the first case lapsing from consciousness and becoming latent.

Modern times give us many examples of such unresolved and active suspicions becoming persistent between nations, classes and groups of men. For here neither can remove itself from the neighbourhood of the other; neither can refuse all intercourse with the other; neither can resolve its doubt and evil surmise of the other. No action of governments, no boards of conciliation, have yet been able to remove these suspicions, and replace them with the generous emotion of confidence. More economic than biological, they poison modern social life, destroying all harmonious co-operation between classes, and giving them that restless, discontented and even revolutionary tendency which is exhibited all over Europe to-day. In this its latest social developments Suspicion is always, in its class-consciousness, thinking evil of the class it suspects. And here, because it is so persistent, it can hardly remain alone, but generates a sentiment of hatred.

It is in such cases where Suspicion is neither displaced from consciousness nor resolved into one or other of its components, that we can most clearly distinguish the characteristic behaviour of this most elusive emotion, and the result which it is organised to achieve. And here we must first recognise that this behaviour is always in the nature of a preparation for something else to follow it, and apart from such a possible consequence, has no significance and no utility. Hence it is the nature of Suspicion to invent that general, deliberate and secret preparation for evil eventualities which is possessed by no other emotion. This behaviour is its end. For Suspicion has an end It is a system. It is not a mere aggregate of other emotions. When it is resolved into one or other of these, we no longer have Suspicion. When it is displaced, it is no longer present in consciousness. And with regard to the question we raised whether the Repugnance-tendency which here operates is not the fundamental tendency of Suspicion, we must answer, No. For it is only when Suspicion, however strong on the side of its conviction of evil intent, is emotionally weak, that we follow Repugnance, and avoid the suspected person. Here the emotional weakness of Suspicion leads to its being displaced, and Repugnance takes its place, so that the general preparedness which is alone the distinctive behaviour and end of Suspicion, does not take effect. But wherever Suspicion is emotionally strong, because the evil apprehended concerns ourselves or those whom we love, its characteristic behaviour is usually evoked. As regards the emotion itself, we have judged it probable, notwithstanding its great biological importance, especially for primitive man, that it is not a primary emotion, but is generated by the subconscious interaction of those emotions into one or other of which it is so frequently resolved.

(*Manuscript received* 2 *May*, 1922.)

A NOTE ON LOCAL FATIGUE IN THE AUDITORY SYSTEM

By F. C. BARTLETT and H. MARK.

(*From the Cambridge Psychological Laboratory.*)

In a recent contribution to this *Journal*[1] Mr J. C. Flügel discusses the effect upon binaural localisation of a period of uniaural stimulation. He explains the apparent displacement of a tone which is thus produced wholly in terms of local fatigue. "The fatigue," he says, "manifests itself in an apparent displacement of the tone towards the unfatigued side[2]."

In this note we propose to confine ourselves to pointing out that the observable facts are somewhat more complex than Mr Flügel has demonstrated, and that in consequence his explanation cannot be accepted as adequate.

For our experiments the observers sat in the sound-proof room of the Laboratory. As source of sound we used a Stern *tonvariator* giving a tone of 157 vibr. per sec. This was placed outside the observer's room, the sound being conducted to the listener through a tube. We considered it desirable to use as simple a type of apparatus as possible for producing the necessary adjustments of the length of path travelled by the sound waves to the ears, and accordingly employed precisely the same sort of instrument as that used by Myers and Wilson in their research[3], reducing it in size for our purpose. Our observers wore specially constructed sound-proof head gear, with stethoscope tube endings which fitted into their ears. This head piece proved both efficient, and (a point of no small importance) comfortable.

In actual procedure our experiments followed Mr Flügel's arrangement exactly, save that we added one further period to those investigated by him, and throughout used only a method of presentation. Thus we had:

(*a*) a period during which the subject's 'normal centre' was ascertained—normal period;

---

[1] XI, 105–34. [2] *op. cit.* 133. [3] This *Journal*, II, 363–85.

(*b*) a period during which his centre after 'fatigue' was obtained—fatigue period;

(*c*) a period during which his centre after 'adaptation' was investigated—adaptation period.

During the 'normal' period the subject simply gave ten successive judgments—the experimenter adjusting the apparatus—as to the point at which the sound apppeared to him to be central. In five of these the sound was made to approach from the right, and in five from the left.

During the 'fatigue' period the observer repeated this task, having been previously subjected to continuous uniaural stimulation for three minutes. The one-sided stimulation was secured, not by pinching a tube—which we do not believe to be a satisfactory method—but by actually breaking the pathway to one ear. When it is remembered that the subject was in a sound-proof room, and wore sound-proof ear-pieces, the sound itself being produced outside the room, it will be seen that even the smallest external stimulation of the resting ear by the test sound was adequately guarded against. We added the 'adaptation' period definitely to put Mr Flügel's local fatigue hypothesis to the proof. The procedure of the ten central judgments was repeated, but in this case was preceded by a 3 minute interval during which *both ears were being continuously stimulated*, but the apparatus was set by the experimenter in such a position that the sound appeared to the observer to be wholly right or wholly left.

Five observers took part in the experiment. All were adults, three being men and two women. Two—*A* and *B*—gave ten sittings, and three—*C*, *D* and *E*—gave five sittings. The order of the tests was varied, thus:

1st day: normal, fatigue, adaptation.
2nd ,, fatigue, normal, adaptation.
3rd ,, adaptation, normal, fatigue.
4th ,, fatigue, adaptation, normal.
5th ,, adaptation, fatigue, normal.

Each subject was given adequate practice, and worked throughout entirely without knowledge of his results. A resting, or recovery, interval of a few minutes separated each stage of every day's work.

We will take 0 to represent an observer's normal centre, and use a + sign to indicate displacement to the right, and a – sign to indicate displacement to the left, the results being expressed in terms of millimeter adjustments on the apparatus. Working out mean centres for each period for our five subjects we have:

	After 'fatigue' of right ear	After 'fatigue' of left ear	After 'adaptation' of right ear	After 'adaptation' of left ear
A	+3	−2	+3	+0·3
B	+1	−5	+1	− 3
C	+4	−3	+8	− 4
D	+8	−6	+10	− 3
E	+3	−6	+4	− 5

In every case but one, that is, a preliminary period of 'adaptation' produced displacement, sometimes to a slightly greater, sometimes to a slightly less, and sometimes to the same degree, as a preliminary period of 'fatigue.' The one exception is *A*, whose results show a very slight displacement to the right after 'adaptation' of the left ear. But this result looks as if it may fairly be taken to be unusual, and in any case the displacement is extremely small in amount. In five out of his ten sittings in fact, subject *A* showed a very slight leftward displacement after adaptation of the left ear. In all the other subjects the direction of the displacement remained remarkably consistent alike after fatigue and after adaptation.

The conclusion appears perfectly definite that displacement of the type discussed by Mr Flügel may equally well be produced after binaural stimulation with one-sided localisation, as after uniaural stimulation. As the adjustment required to effect the one-sided localisation is not sufficient to give appreciable differences of intensity in the tones affecting the right and left ears respectively, local fatigue cannot possibly be the explanation of displacement after adaptation. And since the factor of one-sided localisation is present in the so-called 'fatigue' tests just as it is in the 'adaptation' tests, we are forced to conclude that local fatigue may fail to be an adequate explanation there also.

No doubt diminution of local sensitivity to a sound stimulus may occur as a result of continuous uniaural stimulation. But our results show that this cannot be the whole explanation of the matter. Every observer possesses what may be called an 'indifference area' in regard to the binaural localisation of sound. Within this area movement is detected with difficulty, and only when it is of relatively great extent. Outside the area the smallest movements may be easily appreciated. Analysis of our results indicates that in certain rather rare instances what occurs is simply an extension of the boundaries of this area in both directions, though under the conditions of the experiments such extension is generally more marked on one side than on the other. Far more frequently however the whole area appears to move leftwards or rightwards as the case may be, such movement being capable of experimental control. But we seem to have to ascribe both types of displacement to an 'error of judgment,' rather than to mere one-sided decrease of sensitivity. Whenever any series of judgments has to be made the nature of a given member of that series may depend chiefly on its position in the series. Perhaps the clearest and most interesting illustration of this which has been fully investigated is the case of motor attunement[1]. These displacement results afford, in our opinion, a very pretty illustration of the principle of attunement. The extreme right or extreme left position having been continuously presented for some time, a 'set' is established as a result of which a marginal position appears more central than it normally does.

The comparatively rare case of an extension of the indifference area in both directions does not fall under the same explanation, but appears to be a 'confusion' result due to certain special conditions which require further study.

Clearly the attunement hypothesis should be as capable of exact test as that of local fatigue. In this and in several other ways the experiments briefly described in this note open up an interesting field for further research.

---

[1] See Myers, *Text Book of Experimental Psychology*, 2nd edition, Cambridge, 1911, pp. 207–9.

(*Manuscript received* 27 *August*, 1922.)

CRITICAL NOTICE

BY CARVETH READ.

Territory in Bird Life, by H. ELIOT HOWARD. London: John Murray. 1920. pp. xiii + 308. Price: net.

A most instructive and entertaining book, full of observations and ideas, a model of the books that are wanted for the study of animal minds. If there were similar descriptions and analyses of the lives of Amphibia, Reptiles and Mammals, Comparative Psychology would be in a more prosperous state. But the habits of other vertebrates are far more secret and retiring than the life of Birds; who, secure in their winged freedom, lend themselves to observation.

One characteristic of Bird life, the plainly-marked division of their year into breeding-season and season of miscellaneous wandering and feeding, forces it upon everybody's notice from childhood; and it is generally known that in the wandering half-year most kinds of bird are to be seen feeding together in mixed flocks or small parties—very conspicuously in the case of starlings and several finches. The nesting-season is ruled by a chain-instinct, of which each link (building, incubating, etc.) is itself a highly organized instinct. And the central interest of the whole is so plainly the pairing of the old birds and the rearing of the young, that all the conspicuous activities of fighting and singing and migrating have been explained by referring them directly to that interest. But in this book Mr Howard shows by long-sustained and systematic observation that singing, fighting and migrating are really related, in the first instance, indirectly to the pairing and rearing, but directly to another factor less noticeable to the ordinary spectator: a condition without which reproduction in all its stages could not be efficiently carried out. That factor is the 'securing of a territory' by the male birds, wherein to build the nest and to find food (there, or in the neighbourhood) for both parents and offspring during all the stages of the breeding season.

Early in the year, the male birds of most resident species leave the feeding-grounds, where they have been accustomed to associate with others both male and female and often of several species and, resorting to some suitable station (perhaps where last year they bred or were bred), select some perch on tree or bush, or some patch of open country (according to their specific needs) and there sit and sing, or tower above it singing. At first they go there only for a short time morning and evening, passing midday at the old feeding ground; but soon they occupy it constantly. It becomes their headquarters for the breeding season. Flying about in the immediate neighbourhood for food and always returning to their perch, the distance and direction of these habitual excursions mark out the territory they claim. Within it they sing, advertising their presence and possession to all competitors and to any female of their species that wants a mate, and from it they drive away other males that threaten to raise conflicting claims too near for the welfare of the family. When migrants arrive later in the spring, the males (who generally come first) also establish territories—at once and with no tentative interval of preparation, as they have no time to lose.

The author excuses his use of such a word as 'territory' with its human associations; but its entirely birdish use once explained, I see no objection to it. Mr Howard, as I understand him, never supposes that a male bird in establishing a territory foresees (at least in its first breeding-year) the future course of events: his action (to adopt Prof. Lloyd Morgan's distinction) is *purposeful* (in Nature) but not *purposive* (in consciousness). Yet he behaves as if he foresaw: sings, and thus advertises his presence; fights to make good his claim. It is very instructive to notice that we have here a strong interest in something which in itself is uninteresting; for except proleptically with reference to a mate and nestlings, this half-acre of woodland or meadow is of no more value to the bird than any other spot. It is instinctive behaviour similar to the action of a savage when making an arrow, to which he gives his whole mind, although by itself the object is nothing. It is true the savage knows the uses of his arrow; but the knowledge would not maintain his diligence if the work had not become intrinsically interesting; and this reappears in the play of our children. Whatever is necessary to a necessary end must itself partake of the character of an end, and an example of this is the bird's defence of his territory. How the savage gradually acquired his interest in arrow-making we can imagine; but how did the bird acquire his interest in a pitch?

The male bird, then (1) secures a territory, which is necessary to the adequate feeding of himself and mate and young ones, where food may be got quickly enough after the young are hatched to make it easy to feed them without being long away; for they cannot bear much exposure, especially at temperatures below 51° F. (p. 188). There (2) he sings, and (3) fights intruders. There (4) the female finds him; (5) they pair; (6) build their nest; (7) incubate the eggs; (8) rear the young (perhaps more than one brood); and (9) disperse and return to common feeding grounds. Such is the chain of activities, starting from the securing of a territory, and each implying all the rest, and probably all unforeseen by a bird in its first breeding year.

The song of the male bird is not, in the first place, directly addressed to the female to charm her, nor is it rendered to show his superiority to others; for it begins some time before she comes, and after her arrival often ceases for a period. Its first use is to advertise the possession of a territory; and, generally, it is only in or above the territory that the song is heard; though it is true that a few species sing in Autumn, apparently in play, when the breeding season is over. Its second use is to let females know where to find a mate with an establishment, and this gives occasion for its specific character, that they may know not merely that some bird holds a territory, but of what kind he is. Hence the endless variety of bird-song. Its function in charming and exciting the female (which in some cases, surely, is undeniable) is therefore secondary. Mr Howard has an interesting theory of the development of songs from the call-notes, which differ much less from one species to another than the songs do. This was casually suggested by Darwin (*Descent of Man*, c. xiii, 2nd ed. pp. 378–9); but our author's elaboration of it is his own. He does not offer it as a complete account of the wonderful diversity of their often complex strains.

Similarly, the fighting that may often be seen amongst birds, especially in Spring, is not in the first place for the female, but for the territory; for, again, it begins before she arrives; and it is witnessed not only between males, but sometimes between pairs, or even between females who join in the dispute about the territory. Moreover, fighting occurs between birds of different species, and this cannot be about a mate, but is still concerned with the territory; for whilst birds of the same species (having the same wants) are most immediately in competition, all those that have similar feeding habits are natural competitors. That the food supplied by, or in connexion with, the territory determines its value, is shown by the extent of land that satisfies different species; for whilst a Falcon needs some square miles, a Reed-warbler is content with half an acre, and a Guillemot with a few square feet of rock, because his feeding-district is in the sea below the cliff. I suppose there is a similar explanation of the close nesting of Rooks and still closer association of South African Weaver-birds: abundant food in a circuit narrow enough for a strong flier. Finally, it may be noticed that a bird fights vigorously and pertinaciously hardly anywhere outside his own territory. Hence much fighting amounts to a hardly serious squabble, in which one of the birds (being out of his ground) is not really pugnacious, so that such encounters may sometimes be mistaken for play. That birds fight for breeding-grounds has (I think) been mentioned by Altum; but this whole theory of the importance of the territory in bird life is (as far as I know) original.

Out of this important doctrine of 'the territory' there arises a theory of the origin of Migration. Mr Howard observes that most resident birds are more or less gregarious in Winter, when gregariousness contributes to their safety from birds of prey and facilitates the finding of food; but on the approach of Spring the males begin to leave the flock, and go (it may not be far) to find a breeding-ground. This is in fact a migration; for migration cannot be defined by the distance traversed. Some birds move from one part of our island to another, some to the South of Europe, some to the opposite hemisphere, and others merely cross the hedge; but it is the same sort of action, dividing the common feeding from the breeding territory. In seeking a breeding-ground birds are disposed to return to their birth-place; if this is occupied they must go a little further; and so the pioneers of a prosperous species will in course of ages go a long way; and supposing that they are also disposed to return to the old common feeding-ground, their Summer and Winter quarters will become ever wider apart. Since it seems to Mr Howard that Migration may in great measure be explained by these simple considerations, he is sceptical as to such hypotheses as Wallace's, which traced Migration to the influence of the Glacial Period. Migration may have (and on the territorial hypothesis must have) existed long before the Quaternary Age. However, I venture to think that such extreme and extensive changes of climate as happened then may reasonably be supposed to have had some influence on Migration: it is not necessary to attribute all its phenomena to one only cause. Glaciation of the northern world may well have established by selection a disposition to wider dispersal, especially in those birds that cross the tropics. It is difficult to understand how that can have happened merely as an extension of a quest for territory neighbouring an old home; nor can this quest explain a crossing of the sea (but, of course, many birds that migrate across seas had formerly a route by land); nor why the direction of Migration is chiefly north and south.

All lovers of birds or students of animal life must thank Mr Howard for his book: communicating to the envious reader the joy of his own intimacy with birds, it is not less delightful than instructive.

PUBLICATIONS RECENTLY RECEIVED

Visuell Wahrgenommene Figuren. By EDGAR RUBIN. Berlin: G. Boghandel. 1921. Pp. xii + 244.

This study raises in an interesting and distinctive form the question of the relation of the experienced object to the presented object. Given a green field on a black ground, either can be experienced as 'object'; and so with fields and grounds in general. Hence from the same objective stimulus there are drawn two types of experience, so different as usually to exclude recognition, but yet quite objective. There thus emerges, with unusual concreteness and vividness, an order of middle terms between experiencing subject and presented object—the order of experienced objects; through which stimulus and sensation, which are at present undoubtedly very wide apart, may yet be brought together, and their very opposition derived from a really fruitful theory of perception.

Rubin himself does not work out in any detail the theoretical possibilities of his chosen field. But in practice he drives in very deeply the wedge between presented object and experienced object. He finds very naturally that the alternative chosen in the learning process tends to reimpose itself in the recognition test. He calls this tendency of what is experienced once as figure to be experienced as figure again, the "figural after-effect." If by deliberate adjustment in advance it is counter-acted (with difficulty), recognition is prevented.

In a second section he considers the perceptual relations of contours and lines to the surfaces they enclose, and arrives here too at the conclusion that these are separate experienced objects—though inseparably one presented object; that the surfaces, as experienced objects, have breadth, etc., independently of the distances between the enclosing lines; and that the lines in their turn as objects can be experienced without breadth (though with colour).

Some of these conclusions are debateable, and the experiments are far from being exhaustive. But the study deserves considerable credit for bringing into due focus, with admirable clearness, an important and valuable field of psychological inquiry, which can scarcely receive too much attention. A number of beautiful illustrations add greatly to the book's effectiveness.

Die Grundlagen der Psychischen Entwicklung. Eine Einführung in die Kinderspsychologie. By Professor K. Koffka. 1921. Osterwieck am Harz: A. W. Zickfeldt. Pp. vii + 278. M. 36.

Professor Koffka's book is in the first place a general introduction to child psychology, intended as a first-hand account (chiefly for teachers) of the present state of the subject. It utilises a great deal of interesting recent work, and aims all the time at the due subordination of the detailed facts to the general problem of individual mental development. The treatment is broad, the presentation simple and clear; the critical notice of opposing views is reasonable and fair; and up to a point the book admirably fulfils its popular purpose; though little more.

At a certain stage in the exposition, however, Professor Koffka's interest in the general problem of mental development begins to encroach upon the rest of his theme, for as he has a special theory to vindicate, at the expense of others in present favour, the task of criticism and refutation naturally attracts the most part of his attention. As a consequence, the balance of the book is disturbed, and the volume is largely turned from its purpose, many important things at the close being hurried over with scarcely a mention. The book therefore cannot be recommended as a general introduction to child psychology. On the other hand, it stands as a significant expression of a recent movement of some importance in German psychology, in which Kohler's is the most prominent name, whilst Wertheimer and Koffka himself have also been active.

Under the first heading of Method, Behaviourism is inevitably dealt with, and some concessions are made; but Introspection as an indispensable second source of data is retained. It is, however, best named, not "innere Wahrnehmung" or "Selbstwahrnehmung," but "Erlebnis-Wahrnehmung."

A distinction having been made between maturation and learning and the original equipment of the newly-born infant surveyed, the author settles down to the process of learning. Here he finds the first achievement a distinct problem, which must be separated from that of retention, being quite as important; and he rejects Thorndike's and all analogous theories in favour of a definite, transforming, novelty-creating 'insight.' He relies mainly on Kohler's experiments on chimpanzees, of which he gives a fairly complete account, and which certainly make a formidable body of evidence.

But, on his view, 'insight' is not reasoning, or even intelligence, in any abstract sense. It is of the type of all mental activity: which is *structure formation.* The same objections are found to apply to the traditional sensations, to associations, to reflexes, and in general to all particulars and explanations based on external connexions between particulars; and the single answer to all these objections, the continuous passage from the simplest to the most complicated forms of knowledge, the unit of cognition, is found in the concept of structure-formation. It is exceedingly significant that the primary structure type, which is to take the place of the traditional chaos of sensations, turns out to be the theme of Rubin's study: a quality on a ground. The new theory of knowledge is to be based on perception instead of sensation.

Unfortunately the concept of structure is left rather vague. It is by no means clear that the quality-on-ground is a structure in the same sense as that needed for insight, reflexes, etc. An attempt to show this would be quite indispensable. In general, Koffka does not carry discussion and analysis far enough to be convincing, even where he produces the impression that he may be right. His original purpose in this book is here in the way. But there can be little doubt that the theory of structure will have to be reckoned with; and so much Professor Koffka's book can fairly claim to make clear.

Psychology: A Study of Mental Life. By Professor ROBERT S. WOODWORTH. London: Methuen & Co., Ltd. 1922. Pp. x + 580. 8*s.* 6*d.* net.

Here is yet another attempt at an elementary text-book of psychology. It is a good one, and for three main reasons. First, the order of arrangement is excellent. The book begins by a study of various types of reactions, leads on to a discussion of native and acquired traits, instinct, emotion and feelings, and only later turns to a more detailed study of sensory processes and to perception, memory, reasoning, imagination and will. Secondly, the material is well chosen, and is very much more *au courant* with recent research than is the case in any other book of the same type. Thirdly, the presentation of the material is lively, interesting, and well-adapted to stimulate further study.

Introductory Lectures on Psycho-Analysis. By Professor S. FREUD. London: George Allen & Unwin. 1922. Pp. 395. 18*s*. net.

This is a translation of a series of lectures given in Vienna, and published in 1917. In many respects it is the most important of Freud's works hitherto translated into English.

It is divided into three parts, the first two of which, on Errors and Dreams, are more elementary in character than the third—on Neuroses.

The treatment throughout is masterly, and at no point is one left in doubt as to the meaning of the writer, however much one may disagree with the conclusions which he draws. As usual it is with a sigh of relief that we turn to his own words from a perusal of that mass of literature produced by his followers. Nowhere do we find the dogmatism and disregard of possible objections with which we have become only too well acquainted in other writers. Here Freud deals with and answers the objections which are brought against his theory,—objections which occur to most readers, and which demand a reply.

For more advanced students the last part is of chief importance, for it is there that Freud puts forward, in a tentative form, the views on 'Narcissism' and 'Transference' which have widened the field of Psycho-analysis, and considerably modified its more extreme views.

The Misuse of Mind. By KARIN STEPHEN. London: Kegan Paul Trench. 1922. Pp. 106. 6*s*. 6*d*. net.

To express the philosophy of Bergson in simple language is not an easy task, but it has been achieved with admirable effect by Mrs Stephen. In the first chapter two ways of knowing are described: (1) the knowing of common sense, having for its object abstractions like "solid tables, green grass, anger, hope, etc.," and (2) knowing-for-knowing's-sake which has for its object 'actual fact.' There is also postulated a "mysterious field of virtual knowledge" which we are said to know "in some sense" already, but which we can only know-for-knowing's-sake, if we can intuite. In the second chapter Mrs Stephen proceeds to the description of the 'actual fact,' back to which the strayed sheep of intellectualism are being led, only to find it strangely unfamiliar—a duration made of matter and memory. The theory of matter and memory, dealt with in the third and last chapter, supplies the solution to the problem of why the fictions of common sense are endowed with such surprising validity. Particularly valuable is the frank way in which Mrs Stephen confesses the uselessness of language to express Bergsonian doctrines; whatever may be one's verdict as to the tenability of the doctrines themselves, at least Mrs Stephen has prepared the way towards a clearer understanding of their tenets.

Suggestion and Mental Analysis. By DR WILLIAM BROWN. London: University of London Press, Ltd. 1922. Pp. 165. 3*s*. 6*d*. net.

Dr Brown here deals with 'mental analysis' and 'suggestion,' and discusses the relation between the two. He discusses Freud, Coué, and finally Bergson. He records a number of interesting 'cases' from his own experience and considers their significance for psychological theory. The book has undoubted interest, but is more popular than thoroughgoing. The last part, entitled "The Philosophical Background," seems to have little connexion with the earlier pages.

The Poetic Mind. By Professor F. C. PRESCOTT. New York: The Macmillan Co. 1922. Pp. xx + 308. $2.00.

This book merits a much more extended notice than can be given to it here. It is a thoroughly interesting and well executed piece of work. Professor Prescott has taken for his general field of investigation a study of the mental processes involved in artistic, and particularly in poetic, creation. He holds that we normally employ two modes of thought, the one rational, analytic, conscious; the other visionary, associative, unconscious. Imagination has its place in both, but as it belongs to artistic construction is chiefly identified with the second. With copious and striking illustration these two modes of thought are studied, their conditions considered, their products exhibited. There is no chapter in the book but is well worth careful reading, and new light is shed upon a variety of very old problems. Apart from its very great psychological interest, the book is delightfully written, and a pleasure to read.

The Effect of the Physical make-up of a Book upon Children's Selection. By FLORENCE EILEEN BAMBERGER. Baltimore: Johns Hopkins Press. 1922. Pp. viii + 162. $2.00.

This monograph is an exhaustive study of the influence of various characteristics of a book, such as size, colour of binding, nature of illustrations, arrangement of type and so on, upon children's preferences. The conclusions, although not startling, are interesting, and should be useful for all who are engaged in the preparation or the choice of books for young people. As is perhaps generally suspected, it appears that the adult's estimation of the book preferences of young children is "most inaccurate."

Free Will and Determinism: An Old Controversy and its present Position. By J. RAYMOND SULLY. London: Constable & Co. 1922. Pp. 27. 2*s*. 6*d*. net.

This small book presents a series of short extracts from the writings of various authors on the subject of free-will.

First Report of the Miners' Nystagmus Committee. Medical Research Council Special Report Series, No. 65. London: H.M. Stationery Office. 1922. Pp. 64. 1*s*. 6*d*. net.

It is concluded in this report that "the essential factor in the production of miners' nystagmus is deficient illumination." The reasons for this deficiency are carefully considered and recommendations are made for its treatment. The distribution of nystagmus among different classes of workers is carefully investigated, and the relation of nystagmus to the psycho-neuroses especially considered. The general report is the work of Dr T. L. Llewellyn; Dr J. S. Haldane deals with the subject of Mine Gases; while the note on Psycho-neurotic Symptoms associated with Miners' Nystagmus is the work of the late Dr W. H. R. Rivers.

The Machinery of the Mind. By VIOLET M. FIRTH (Foreword by A. G. Tansley). London: George Allen & Unwin, Ltd. 1922. Pp. 95. 3*s*. 6*d*. net.

This is a very popular account of modern psychology. It is interesting, and as an introduction to further study may be recommended.

La Religion et La Foi. By HENRI DELACROIX. Paris: Félix Alcan. 1922. Pp. 462. 25 fr.

In the first part of this book the author describes the positions taken up by religious belief in its flight before growing intellectual powers. In the second part the more violent religious experiences are dealt with, and in the last and most instructive part an account is given of conversion to and from religious belief. Finally M. Delacroix speculates on the subjective causes of religion and links them up with a hint at a Universal Soul.

The work displays great learning, but lacks unity and plan; the terminology is general and undefined; and the train of thought is often confused. Religions other than Christianity only receive a passing mention. The descriptions and analyses of mental states are too superficial to be of any great value to Psychology.

A Handbook of Mental Tests: A Further Revision and Extension of the Binet-Simon Scale. By Dr F. KUHLMANN. Baltimore: Warwick & York. 1922. Pp. 208.

Apart from its extensive revision of tests, this book is worth reading for its very interesting discussions of the "General Principles of the Year Scale," and "The Conduct of an Examination." The revision itself seems judicious and useful.

Philosophical Studies. By Dr G. E. MOORE. London: Kegan Paul. 1922. Pp. viii + 342. 15*s*. net.

This very attractively produced book consists of ten essays on various philosophical and ethical problems. All but two have been published before, but they are of first-rate importance, and it is well to have them gathered together into a single volume. The series which this volume initiates is a publishing venture of very great importance. Dr Moore's book sets a high standard for successors.

The Growing Girl. By EVELYN SAYWELL (preface by Dr H. Crichton Miller). London: Methuen & Co. Pp. vii + 37. 1*s*. net.

These short lectures are mainly directed to helping parents and teachers to deal with the interests of the growing child. They are sane, and, so far as they go, practical.

Emile Coué, The Man and His Work. By HUGH MACNAGHTEN. London: Methuen & Co. Pp. xi + 52. 2*s*. net.

Mr Macnaghten visited Coué at Nancy and consulted him for severe insomnia. He received help, and wrote these short papers to express his thanks.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY.

GENERAL MEETING.

July 15, 1922. Experiments on Musical Appreciation, by C. S. Myers.

Some Experiments on the Process of Conventionalisation, by F. C. Bartlett.

Some Experiments on Contrast Effect in a Smoothly Graded Disc, by R. H. Thouless.

SECTIONAL MEETING.

Industrial Section.

July 15, 1922. The Value of Curves of Output, by Eric Farmer.

The Speed of Human Muscular Movement, with a Demonstration of the Inertia Ergometer, by A. V. Hill.

The Effect on Mental Fatigue of Varying the Duration and Quality of Rest Pauses, by S. C. Jackson and S. Wyatt.

VOLUME XIII JANUARY, 1923 PART 3

AN ANALYSIS OF LITERARY APPRECIATION[1]

BY OLIVE A. WHEELER.

(*From the Department of Psychology, University of Manchester.*)

CALCUTTA UNI
Sec. No.......
Book No........
LIBRARY

I. *Introductory* (pp. 229, 230).
II. *The image-formation method* (pp. 230–232).
III. *Experiments on the image-formation method* (pp. 233–238).
IV. *General conclusions* (pp. 238–242).
V. *Critical consideration of current methods of teaching poetry in the light of the foregoing analysis* (p. 242).

I. INTRODUCTORY.

THE recent Report on the Teaching of English[2] has perhaps done more than any other publication to re-inforce the almost self-evident truth that the study of English literature should occupy the first place in the curricula of all types of English schools. As a result of this report, and of the interest which it has aroused among all classes of teachers, it is not unlikely that in the immediate future more time will be devoted to the study of English than has been usual in the past; and the question of the methods to be employed in its teaching has therefore to-day become one of great urgency.

It will be generally agreed that one of the main objects of the teaching of English in schools is to help the pupils to *enjoy* good literature, so that it is to them for ever "a possession and a source of delight," "an end in itself[3]" as well as an equipment for the understanding of life. The process of appreciation or enjoyment is of course always individual and organic. It is the individual who enjoys; and the work of art which is enjoyed is in a sense unique and unanalysable. Yet if the general conditions of literary appreciation could be discovered, although these might not apply equally to every case, they would at least suggest methods of teaching suitable for the great majority of children. It is in the hope that some such conditions may be discovered that this analysis of literary appreciation has been undertaken.

At the outset, it is perhaps desirable to guard against a possible misunderstanding. The fact that the process of appreciation is specially under consideration must not be interpreted to mean that an individual's enjoyment of literature is entirely unrelated to his attempts at creative expression. The appreciation of the work of a great artist may inspire the student to a fuller creative expression of himself: and conversely, his attempts at expression in the same medium, however imperfect these may be, may intensify his understanding and enjoyment of the master's art. Such interaction between appreciation and creation is possible at all stages of development; but there can be no harm in momentarily setting it on one side for the sake of the clear delineation of the factors in appreciation which would otherwise remain unrecognised.

---

[1] Read at a Meeting of the Education Section of the British Psychological Society, 9 May, 1922.

[2] *The Teaching of English in England* (published under the authority of His Majesty's Stationery Office, 1921).

[3] *Ibid.* p. 19.

Although a psychological analysis of literary appreciation, and particularly of the appreciation of adults, is necessarily only a preliminary stage before the practical testing of the method or methods which it suggests upon boys and girls of different ages, it may yet prove useful to educationists in at least two directions. In the first place, it may shake a too confident belief in the infallibility of the older methods of the presentation of poetry, such as silent reading, critical exposition and parrot-like repetition. Even a teacher of literature, who is concerned with living children and a living art and the inner meaning of the life itself, may mechanize his method of teaching, and may come to regard it not as an imperfect sketch requiring constant retouchings in the light of a more extensive knowledge of children and a deeper understanding of life, but as fixed and static, the one and only infallible method. Any analysis which disturbs this false security is not without value. In addition, an analysis of literary appreciation may very naturally be expected to give guidance in the selection of those new methods of presentation, which it would be worth while to test experimentally under class-room conditions.

Let us then attempt to discover what distinguishable factors appear to enter into the appreciation of a poem. Under what general conditions is the reader best able to follow where the artist has led, to grasp what he has grasped, and to see with him into the inner life of things?

II. The Image-Formation Method.

It will be generally agreed that a critical attitude of mind or an analytic consideration of the author's meaning and consistency, although perfectly appropriate for the interpretation of scientific or philosophical literature, does not lead directly to the enjoyment of poetry. What seems to be needed is rather a sympathetic grasp or an intuitive understanding of the movement of the poet's thought; and, in general, this seems to be more readily achieved through clear and vivid imagery than through abstract and conceptual thought.

But the function of imagery in the appreciation even of imaginative literature is by no means so simple as it appears at first sight. From a number of experiments with boys and girls of different ages Mr E. Allison Peers[1] was forced to the conclusion that although in literature where the meaning is contained in the imagery, or where the imagery plays a large part in the author's thought, strong imagery is of great value in appreciation, yet where the imagery is not essential but merely enriches or develops the thought, strong imagery is often a positive hindrance to interpretation. He therefore advises the use of exposition, as opposed to mere reading, in the former case, in order that there may be an increase in the number and clearness of images; but in the latter he suggests that "the teacher should not exert himself to develop the imagery of the piece by explanation or emphasis, until after the interpretation had been completed by his pupils[2]."

If, as Peers maintains, there is a type of literature, the appreciation of which depends "almost entirely" upon the subject's imagery, it is difficult to see on what grounds he advocates the method of exposition for its presentation. For the explanation of words and phrases by other words or phrases is a more direct aid to conceptual thought than to the formation of vivid images. If his psychological analysis be correct, then for this type of literature the obvious method to be employed is an 'image-formation' method.

Before such a method could be used in the presentation of a poem, there would have to be a general preliminary training in the formation of the various kinds of images, chiefly visual, auditory and kinaesthetic. In the case of visual imagery and to a less extent of the other chief types, the kind of training given by Cizek[3] in the Vienna School of Arts and Crafts or that described by Mrs Truman in her book entitled *The Child Vision* would be admirable for this purpose. Mrs Truman was mainly concerned with the teaching of composition to boys and girls of from about 8 to 11 years of age: and the general principle on which she worked was that of taking the images which at this stage naturally exist in the minds of the children as the material to be expressed in language. One child was asked to imagine an object or a scene and to describe it to the rest of the class, each of whom then drew a picture to represent the description. The first child then chose the picture which most adequately portrayed what she had in her mind; and a discussion followed on the reason for her choice, and on the difficulties experienced by the sketchers in representing what was described. There would thus naturally arise both destructive and constructive criticisms of the first child's description; and through their consideration most of the *technique* of composition could be easily taught. That indeed was the main objective Mrs Truman had in mind. But in working towards this objective, the children were receiving a training in clear visualisation. The first child had to recall the object or scene vividly before attempting to describe it; and the others had to translate her language into clear images before proceeding to a representation on paper, so that in both cases a definite training was being given in image-formation.

---

[1] "Imagery in Imaginative Literature." *J. of Exp. Pedagogy*, 1913–4, II.

[2] *Ibid.* p. 280.

[3] F. M. Wilson: *The Child as Artist.* (Conversations with Professor Cizek.) 1920.

Now, suppose that a poem in which imagery plays a large part (for example, Shakespeare's Song on Winter) were about to be presented to children who had received some such preliminary training. The image-formation method in this case would consist, first, of an informal discussion of winter as a preparation for the relevant imagery. The children might be asked what the country looks like in winter, what they would expect to see if they were staying at a farm, what people look like out of doors when it is very cold, what they feel like, what they do to try to keep themselves warm, what it would be like indoors. Then would follow the reading of the poem:

> When icicles hang by the wall,
> And Dick the shepherd blows his nail,
> And Tom bears logs into the hall,
> And milk comes frozen home in pail,
> When blood is nipp'd, and ways be foul,
> Then nightly sings the staring owl,
>   To-who;
> To-whit, to-who, a merry note,
> While greasy Joan doth keel the pot, etc.,

and a slower re-reading, with questions concerning what the children can see and hear and feel in imagination; for example, what Dick the shepherd and greasy Joan look like, what they are doing, and so on. The important point is that the kind of questioning should be such as to lead to the formation of vivid images, rather than to the translation of one phrase into another. Thus, one would not ask "What does 'keel the pot' mean?"; for that would naturally lead to verbal explanation, which might be unaccompanied by an image: but rather "What can you see greasy Joan doing?" Finally, the poem would be read straight through again.

III. EXPERIMENTS ON THE IMAGE-FORMATION METHOD.

Obviously this image-formation method could be much more easily carried through under test conditions with educated adults than with children; for in the former case the difficulties of the reading and the interpreting of the actual words would be practically negligible. In addition, with the proper preliminary training, educated adults would be able to direct their own minds to the formation of the relevant images without the intervention of another mind than that of the poet. On this account, the first testing of the image-formation method was made on University students.

The fifty students of the University of Manchester (15 men and 35 women), who completed the following tests, had pursued various courses in the University. There were twenty-two honours graduates, in history (6), French (4), mathematics (4), botany (4), Latin (2), English (1), and chemistry (1): eleven pass graduates (5 Science and 6 Arts): nine 'failed' B.A.'s, or B.Sc.'s: and eight others who had either not pursued, or not completed, a degree course. They were all students of elementary psychology; and they had had considerable training in the formation of the different types of images, for they had all been tested and graded in respect to the clearness of the chief kinds and to their facility in using them.

Experiment I. In the first experiment, the students silently read through a short poem; then re-read it verse by verse, noting on the specially prepared sheet of instructions the images that occurred *naturally*. Then they re-read the poem at their usual pace. Twenty-five of them read Gray's Ode *On a Favourite Cat drowned in a tub of gold fishes*, a poem in which imagery appears to play a large part in the author's thought (this is the poem X in the tabulated results); and twenty-five read Shelley's Ode *To the Night*, where the images serve to enrich but are less essential to the thought (this is the poem Y). After the third reading, the students were asked to state whether in their opinion the noting of the images on the second reading increased (+), left unaffected (=), or decreased (–) their appreciation.

The results were as follow:

Appreciation.

Poem	+	=	–	Total
X	22	2	1	25 students
Y	20	2	3	25 „
	42	4	4	50 „

From the tabulated results it will be seen that 22 of the 25 students, who read X, and 20 of the 25, who read Y, record an increase in appreciation due to the clear formation of the images. That is, 84 per cent. of the total believed that the noting of the images aided appreciation.

The introspections of E. M. and W. E. M^{c}C. are typical of the majority. The former after recording a large number of images (visual, motor, and auditory) on the second reading of Y, affirms that the noting of the images "greatly" increased his appreciation. He says "I could see a series of *moving* pictures almost like a display at the cinema slightly tinted by auditory and motor images. The more clearly I see the things described, the greater is my appreciation."

W. E. M^{c}C. recorded a large number and variety of images on the second reading of X. For example, in verse 5,

> Presumptous maid! with looks intent
> Again she stretched, again she bent,
> Nor knew the gulf between—
> Malignant Fate sat by and smiled—
> The slippery verge her feet beguiled;
> She tumbled headlong in.

he notes visual images of the cat with intent looks, of the cat stretching, motor images of stretching and bending, visual images of the gulf, of Fate ("a grinning woman"), a touch image of the slippery verge ("ice"), a touch image of the cat tumbling in, an auditory image of the splash, a visual image of the wetness, and a temperature image of the coldness of the water. And he sums up as follows: "The images, following each other in rapid succession, seemed naturally to blend finally, verse by verse forming a series of *moving* pictures. To some extent, the images, being very vivid, gave merely an appreciation of the narrative and partially destroyed the appreciation of rhyme, etc., which returned on reading the poem as a whole, the third reading giving a totally different aspect from that received at first."

Of equal interest and significance are the introspections of the minority. There was only one student (L. M. T.) who reported a decrease in the appreciation of the poem X through the noting of the images. A comparison of the images obtained by her with those obtained by W.E.M^{c}C., whose experiences have just been considered, is somewhat illuminating. For example, in verse 5 she obtains a visual image of a milkmaid instead of the cat with looks intent; a visual image of people stretching and bending in Swedish exercises where W.E.M^{c}C. obtained a motor image; visual images of a gulf, of Fate; a visual image of frozen pavements, where W.E.M^{c}C. obtained a touch image of slippery; and a visual image of a diver instead of the visual image of the cat, an auditory image of the splash and touch and temperature images of the cold water. And in general she reports: "The images make the poem 'choppy.' I cannot realise it as a whole. The personification is too strong, it calls up other images which are individual and not subservient to the idea of the cat and the fish."

The same kinds of difficulties were experienced by the three students who reported a decrease of appreciation in regard to the other poem, Y. One (E. S.) complained that irrelevant images caused "the initial feeling of the author to slip altogether out of focus," and another (W. H.), that "the mysticism of the poem was taken away" when the images were made clearer.

The experiences, then, of these students appear to differ from those of the majority. Many of their images are not relevant (*e.g.* milkmaid, diver); and what is more, they do not blend. The visual image of a milkmaid will not blend with the visual image of people doing Swedish drill, in the way in which the visual image of the cat will blend with a motor image of stretching. The images of the minority are too static: they do not move with the author's thought: they are more like the successive fixed pictures of the magic lantern than the moving pictures of the cinema. And therefore the poem becomes "choppy," or "loses its mysticism" and atmosphere.

The success of the majority and the failure of the minority, however, both point to one and the same conclusion; namely, that another factor than vivid imagery enters into appreciation. The images must move: they must blend: they must grow and interpenetrate rather than merely succeed one another. There must be continuity, 'duration' in the Bergsonian sense of the term, as well as strong imagery: and there must be a balance preserved between the two factors. The majority of the students succeeded in the first experiment because they preserved that balance: the minority failed because they over-emphasized the imagery factor and partly sacrificed to it the continuity factor.

Experiment II. The second experiment was devised to test the effect of diminishing the continuity factor still further.

The same course was followed as in Experiment I, except that on the second reading the students were asked to *make an effort* to obtain relevant images, not merely to note the images that occurred naturally. The twenty-five students, who previously read X, now read Y: and *vice versa.* At the close, the students were asked (i) whether the attempt to obtain more images than usual increased (+), left unaffected (=), or decreased (−) their appreciation: and (ii) whether there were any cases where the attempt to obtain an image appeared to hinder the appreciation. If so, they were asked to indicate the precise case or cases.

The results were as follow:

Appreciation.

Poem	+	=	–	Total
X	4	2	19	25 students
Y	3	4	18	25 „
	7	6	37	50 „

It will be seen from the tabulated results that in this experiment the majority (*i.e.* 74 per cent.) affirmed that the attempt to obtain more images than usual not only did not help, but definitely hindered appreciation.

Some typical introspections of readers, who had found the imaging a help under the conditions of Experiment I, and who now found it a hindrance, deserve consideration.

T. S. F. (poem X). "There seems to be an adversely discontinuous process which affects the appreciation of the style."

E. G. (poem Y). "The effort to get more images than usual tended to divide the poem up, so that it became jerky and lost its easy flow."

E. H. (poem Y). "The whole poem is dreamy. To attempt to get the images clear interferes with the appreciation of the poem as a dream-like whole."

A. T. G. (poem Y). "The effort to get more images provides sharp angularities in the poem, which is at once misty in outline and mystical in meaning, and it destroys the atmosphere necessary for the proper appreciation of the poem."

Of the four students who found the imaging a hindrance in Experiment I three continued to do so in Experiment II. The fourth (E.S.) now found the clear images a help, but this result is not necessarily contradictory; for E.S. was now working with the poem X where the meaning was much more closely bound up with the images than was the case with Y. Indeed the difference between X and Y is by no means negligible; and therefore Experiments I and II can be best compared by taking the massed results of 25 readings of X and 25 of Y. Viewed in this way it will be seen that under the conditions of Experiment II only 14 per cent., as compared with 84 per cent. in Experiment I, reported an increase in appreciation. In the case of the great majority, the necessary balance between the duration and the imagery factors in appreciation had presumably been lost.

But what of these 14 per cent.? Were they still able to preserve this balance? And if so, why? It is noteworthy that E.M. and W.E.McC., who likened their images in Experiment I to moving pictures and a cinematograph display, are among this minority; in Experiment II they report a similar success in blending and moving the images. Apparently they had still not reached the breaking-point where duration has to be sacrificed to vividness of imagery.

It will be remembered that before the actual appreciation experiments, the students had received a preliminary training in image-formation, through being tested and graded in regard to the clearness of the chief kinds of images and to their facility in using them. And it is significant that, with one single exception, the seven students who record an increase in appreciation under the conditions of Experiment II had been previously graded as above the average in power of imaging, either in general, or at least in respect to one of the chief kinds of images. Thus, one was above the average in auditory images; four in both visual and auditory; and one in visual, auditory and kinaesthetic images. The single exception (E.M.), though by no means deficient in power of imaging, was certainly not discovered in the preliminary testing to be above the average in any one type.

In general, therefore, it would seem that the ease with which the minority could form their images made it possible for them to obtain almost all the relevant images in the poem without disturbance of the duration factor in appreciation; while, on the other hand, the effort which was necessary for the majority stopped the movement of thought and feeling, interrupted its "easy flow," introduced "sharp angularities" into it, and therefore hindered appreciation.

Experiment III. The third experiment was very similar to the first, except that the poem in this case (Gray's *Elegy*, verses 1, 2, 4, 5, 6, 7, 8 and 9) was read aloud by myself, instead of being read silently by the students. In this way the time factor was controlled, the poem being read as far as possible with expression, but without disturbance of its natural psychological rhythm. Thus, for example, verse 1 was read in about 20 secs., and verse 4 in 16 secs. After the third reading, the subjects of the three experiments were asked to state what other factors than the images appeared to have affected their appreciation. Meantime, there had been no comparison nor discussion of the results of the first two experiments.

Only five students answered the above question vaguely, merely stating that the fact that the poem was read aloud aided their appreciation. Most of them succeeded in a greater refinement of introspection. Even the seven exceptionally good 'imagers,' who had succeeded in maintaining some sort of balance between the imagery and duration factors under the adverse conditions of the second experiment, admitted the advantage of hearing the poem read, and were able to indicate other factors than imagery which appeared to them to enter into appreciation. The chief factors explicitly recognised as aiding appreciation were—

continuity of thought (6); a strong and unified emotion or mood throughout (7); the rhythm of the poem (12); the sound of the words (10); the flexibility and modulation of the reader's voice (8); suggestions of meaning and emotional tone directly conveyed by the reader to the hearers (14); actual experiences of country life (3).

IV. General Conclusions.

The first two of these factors, namely, continuity of thought and unity of mood, appear to be already covered by the duration factor, the presence of which was clearly indicated in the results of the first two experiments. But perhaps it may be objected that this continuity would be better termed a mood than a duration. Certainly the apprehension of the poem and the enjoyment of its atmosphere depend upon the sympathetic induction of the dominant mood of the poet in the mind of the hearers. But there is a cognitive side to this unity of mood and feeling. Intimately interwoven with it there is a continuous flow of imagery and meaning: and it is with the intention of covering both these aspects of unity in difference (that is, the cognitive as well as the affective) that I have ventured to use the term 'duration.' The fullest enjoyment of a poem is only possible when the images and their meanings, as well as the emotional tones accompanying them, blend and interpenetrate: and the continuous flow or movement of thought and feeling, which results from this interpenetration, is what I mean by 'duration.'

There is a sense in which a poem may be said to have a life of its own. Like a living organism; it has a unity and yet it develops or unfolds in time. On this account, even the most vivid and varied imagery, gained through a *post mortem* dissection of it, will not in itself lead to appreciation. If the poem is to be truly enjoyed, it must be grasped as a living whole, that is, as a true duration. Indeed, the duration factor in appreciation seems to me to be so fundamental, that I do not find myself able to agree with Peers that there is any case or any kind of imaginative literature, the appreciation of which depends "almost entirely" upon strong imagery.

Closely related to, but not identical with, the duration of a poem, is its rhythm. The poet's thought is a living whole, but it is also articulated internally: for its full appreciation not only must it be grasped as a true duration, but the natural articulations of that duration, that is its psychological rhythm, must be respected. That there is a satisfaction derived from the rhythm of speech, apart from its meaning, is suggested by the fact that it is possible to enjoy hearing poetry read in an unknown language. Indeed, the enjoyment of poetic rhythm, which is a kind of compromise between the metrical measure and natural speech rhythm, appears to depend on the rhythmic pulsation of human thought. Attention comes in pulses or beats; and it is usually supposed that the normal duration of one of these beats is from 1 to 1·5 seconds. Now it is highly probable that when language (and the thought which it represents) is articulated in accordance with these beats, it is especially pleasing to the human mind.

This view is suggested by the work of Mr J. S. Wallace Wallin on "Rhythm of Speech," described in the *Yale Psychological Laboratory Studies* (Vol. IX). From phonographic records he discovered that, in rhythmic speech, a language "expiration group" occupied on an average 1·19 seconds and was succeeded by a silent interval of 0·44 second. The total for the language pulsation, including the interval, therefore works out at 1·63 seconds, the expiration group itself falling well within the normal beat of attention.

It is therefore not improbable that poetry would be much more likely to be appreciated if it were read in such a way that the psychological measure (not of course the metrical foot) were approximately equal to the normal rhythm of attention. Certainly in my own case, the most enjoyable reading of poetry seems to be that in which the pulsations of language, and consequently of thought, agree with the beats of attention. For example, Gray's *Elegy* gains in meaning and atmosphere by being read with due regard for the length of these articulations. In the first verse there are twelve thought pulsations:

> The curfew | tolls the knell | of parting day |
> The lowing herd | wind slowly | o'er the lea, |
> The ploughman | homeward plods | his weary way |
> And leaves the world | to darkness | and to me. |

Each of these should occupy one beat of attention, that is, including the succeeding interval, approximately 1·5 to 1·6 seconds. The whole verse should therefore be read in about 20 seconds: whereas verse 4, where there are only ten thought pulsations should be read in about 16 seconds.

The appropriate reading is therefore rather slow as compared, say, with that of "Sing a song of Sixpence." The first verse of this nursery rhyme actually contains more words than that of the *Elegy*; but it should be read in about 12 as compared with 20 seconds, for it only contains eight thought groups:

> Sing a song of sixpence | a pocketful of rye, |
> Four and twenty blackbirds | baked in a pie |
> When the pie was opened | the birds began to sing |
> Oh wasn't that a dainty dish | to spread before the king.

The reading in Experiment III was spaced in this way: and it is interesting to notice that all the students, including even the exceptionally good 'imagers,' thought that the reading added to their enjoyment; while twelve of their number picked out the rhythm as a factor contributing to their appreciation. One (R.M.) remarked that the way the poem was read "without breaks enabled him to keep hold of the whole." Now as a matter of fact there were silent intervals: but apparently as these occurred between his attention beats, they did not produce discontinuity or breaks in meaning. A second (S.E.) stated that she had a headache but that hearing the poem read was soothing, although thinking about the experiment was not. This soothing effect is evidence of an absence of any strain of attention, due to the fact that the pulsations of thought fell naturally within its beat. Another (L. B.) judged that her appreciation had been increased by the fact that she could not "skip bits" as she tended to do in silent reading. "We were *given* the necessary time," she said, "which we do not usually *take* in reading, to appreciate each part." These results then support the view that for appreciation a poem should be read so that its psychological measure agrees with the normal beat of attention.

Indeed the rhythmical arrangement of words in accordance with the natural pulsation of attention appears to be the special method adopted by the poet for helping his readers to grasp his thoughts as a true duration. In this way they are encouraged to place themselves under the artist's direction, and to follow where he leads. The rhythm factor is therefore fundamental in the appreciation of poetry; and in many cases it probably contributes more to its enjoyment than does imagery.

The same factor lies at the root of the enjoyment of nursery rhymes. Even those rhymes, like "Hey diddle diddle, the cat and the fiddle," which have frequently been rejected by intellectualists as sheer nonsense, do give enjoyment to the little people for whom they were intended. For vital processes themselves are rhythmical: and as soon as enough language has been acquired, the unsophisticated child finds a deep sense of joy in hearing and repeating words that express this natural rhythm. It is true that many nursery rhymes provide little practice in the grasping of a continuity of meaning, that is, of a duration: but perhaps this is not necessarily a disadvantage at this stage. If there be too much meaning for the child's mind to grasp, the rhythm of the words may be violated. In any case, the learning of nursery rhymes can be justified on the ground that it is a simple and natural preparation for the appreciation of the rhythm of speech, without which enjoyment of poetry in later life would be impossible.

A curious case of the accentuation of both the rhythm and the duration factors in appreciation is that of the Welsh *hwyl*. Although this is rapidly disappearing, there are still certain Welsh preachers who adopt this method of carrying their hearers with them. The preacher begins the exposition of his text in an ordinary tone; but gradually as he gets worked up, his words fall into a kind of chant, which is graphically described as the '*hwyl*,' that is, 'full sail.' At this stage, provided that the rhythmical arrangement of words has been arrived at spontaneously and not by mere artifice, the hearers appear to be incapable of criticism. The rhythm acts on them like a spell. They are carried along on the waves of an irresistible tide: they are inspired and uplifted by the experience. Indeed it does not appear to be absolutely necessary that the hearer should understand the language of the preacher in order to experience something of the uplift. It seems rather as if by this method the speaker could communicate his mood to his hearers almost independently of analytic thought processes or even of clear images in their minds.

Now in poetry the rhythm and the duration factors may not be quite so accentuated, but they are of outstanding importance. On this account, any attempt to clarify or to increase the number of images is not likely to aid appreciation, if it cuts across the natural pulsations of the duration of the poem. The images must come within these articulations. They must not be so many or so detailed as to increase the duration of each thought-group beyond the hearer's span of attention. Neither must they be so few or so sketchy as to cause him to attempt to force more than a single thought-group into the unit of attention. But if there be no violation either of the natural beats of attention or of the duration of the whole poem, then strong imagery is an aid to appreciation. For the fullest enjoyment of poetry, the three factors—imagery, duration and rhythm—must not conflict, but must develop in harmony with one another.

With one exception, all the other factors explicitly recognised by the subjects of the experiments as aids to appreciation, such as the sound of the words, the flexibility and modulation of the reader's voice and the suggestions of meaning and mood conveyed by the reader to the hearers, were all directly dependent on the actual method of reading. The one exception is not unimportant. Three students noticed that their previous intimate knowledge of country life was a factor in their appreciation. One (H.R.) stated his belief that if he had not worked on the land his appreciation could not have been anything like as great as it was; his images would not have been so many, nor would they have carried so much meaning with them. This is surely self-evident; but its bearing on the teaching of poetry in schools is sometimes forgotten. The first condition of appreciation, which should be fulfilled by the teacher, is that of choosing for presentation a suitable poem, that is one dealing with a subject which lies within the sphere of the children's experiences and interests.

V. CRITICAL CONSIDERATION OF CURRENT METHODS OF TEACHING POETRY IN THE LIGHT OF THE FOREGOING ANALYSIS.

Suppose, however, that a suitable poem has been chosen. What light does the foregoing analysis of appreciation throw on the method or methods that should be used in its presentation? Most of the current methods of presenting poetry to children do violence to one of three chief factors that have been distinguished. The method of exposition is too critical and analytic, and therefore disturbs the duration and the imagery factors. The methods of silent reading and the reading aloud of the poem by the pupils pay too little tribute to the rhythm factor; while the method of recitation (or repetition) frequently results in the sacrifice of the psychological rhythm to the metrical measure. What seems to be needed, at least for poetry in which imagery plays a large part, is an image-formation method in which justice is also done to the duration and rhythm factors in appreciation. The necessary modification in the method which was described at the outset will obviously have to be introduced through the reading of the poem. For, if the duration and the rhythm factors are to develop in harmony with the imagery factor, the poem must be read rhythmically and with due regard to its meaning. It must therefore be first read by someone who has studied it and is also highly trained in the art of reading aloud. Indeed the reading of the teacher ought to be so artistic as to convey directly to the children the mood of the poem. This would help them more than any amount of exposition to appreciate it as a true duration.

Such being the case, it is surely extraordinary that, in our training colleges and still more in most of our Universities, the study of elocution has become almost entirely divorced from the study of literature. Indeed, it is not unusual to find literature specialists who have taken honours courses at the Universities in English, and have nevertheless received no technical training in the art of reading. If only they had something of this *technique*, they would probably not only revolutionise the art of elocution, but their influence as spiritual teachers would be almost unlimited.

THE EFFECTS OF TENSIONS OF THE BODY SURFACE UPON THE NORMAL DREAM.

By A. J. CUBBERLEY.

I. *Introductory* (pp. 243–248).
  (1) *A dream may be experimentally controlled without assuming an abnormal character* (pp. 243, 244).
  (2) *Previous methods of experiment* (pp. 244, 245).
  (3) *Critical points in dream intensity and dream recovery* (pp. 245, 246).
  (4) *Basis of the tensor method* (pp. 246–248).
II. *Experiments with tensors* (*Series I*) (pp. 248–255).
  (5) *The tensor method technically considered* (pp. 248, 249).
  (6) *Tensor-controlled dreams*—15 *examples* (pp. 249–252).
  (7) *Summary of results—duplicate experiments* (pp. 252, 253).
  (8) *Inferences from the tensor experiments* (pp. 253–255).
III. *Experiments with detensors* (*Series II*) (pp. 255–260).
  (9) *The detensor method technically considered* (pp. 255, 256).
  (10) *Detensor-controlled dreams*—12 *examples* (pp. 256–258).
  (11) *Inferences from the detensor experiments* (pp. 258–260).
IV. *Conclusion* (pp. 260–265).
  (12) *Fundamental data in dream-study* (pp. 260–264).
  (13) *Summary* (pp. 264, 265).

I. Introductory.

(1) At first it may appear a little inconsistent to speak of a normal dream, into which there enters a factor deliberately introduced for purposes of experiment. The two conceptions can be reconciled, however, if we are careful to keep the experimental methods which we use within the limits of disturbance which may be expected to occur in ordinary healthy sleep. Maury's famous experiments, for example, do not fulfil this condition, because for the most part they presuppose relatively large disturbances. How the difficulty is to be got over will be considered in due course. Besides this freedom from excessive disturbance, it is a condition obviously imposed upon the normal dream that the sleeper shall be free from neurosis or psychosis, as those terms are used by recent medical

writers, still more from any grave disease, bodily or mental. If those two groups of conditions are satisfied, then it is the contention of this paper that the study of the dream falls entirely within the province of normal psychology. To maintain this is at once to challenge the position taken up by Prof. Freud and the schools which have evolved from his teaching. According to his views, the dream is a psycho-pathological formation[1], and so (we must assume) is withdrawn from the purview of what has hitherto been understood as normal psychology. I hope to show that this position is untenable.

The following paper is based upon a number of distinct series consisting of about 750 dreams in all, of which all but a few were experimentally controlled. One night was devoted to each experiment: the experiments were arranged in series, each series being designed for the elucidation of a particular point. In a single paper it is only possible to discuss two or three of those series, and those elementary: but within my limits I hope to show, first, that any attempt to establish a genuinely scientific theory of the dream without careful experimental control is futile from the beginning: secondly, that no dream can be 'analysed' with an approach to scientific precision unless the state of the dreamer has been thoroughly ascertained in ways for which Prof. Freud makes no provision: and thirdly, that the controlled dream is an instrument of great potential value for the study of the general problems of mind.

(2) Before we go on to explain the methods to be used, it will help matters if we first take a bird's-eye view of what has already been done in the direction of experiment, as distinguished from the observation and speculation to which most writers on the dream have confined themselves. From various sources it appears that the following four names are outstanding.

Alfred Maury[2] performed on himself, with assistance, tactile and other experiments which have achieved great celebrity. The methods used, however, were coarse, and the resulting dreams cannot be reckoned as normal. They raise complex questions at the very outset of the inquiry.

J. Clavière[3], by experiment with alarm clocks, attempted to prove that the actual rate of dramatic movement in the dream is the same as in ordinary experience. I do not know whether anything more has been done in this direction.

[1] S. Freud. *Interpretation of Dreams.* Eng. Trans. 1921, p. 487.

[2] *Le Sommeil et les Rêves, études psychologiques*, Paris, 1861.

[3] "La rapidité de la pensée dans le rêve," *Rev. Phil.* 1897, p. 507.

G. T. Ladd[1] experimented on the relation between retinal blotches, etc. and the dream. Maury and d'Hervey discussed somewhat similar questions. We shall see that this factor is, in any view, of secondary importance.

J. Mourly Vold[2] experimented with forms and colours, also with tape bandages, etc. The latter ('cutaneous muscular') experiments are given in the work cited, published after his death. For his other writings see Klemm's Preface.

Those four names cover, to the best of my knowledge, all the deliberate experimental work of first-class importance. It is apparent that whether we regard the number of workers or the results achieved, this branch of psychology has been comparatively little developed. The one possible objection to experimental methods, and it is an objection that Prof. Freud has not failed to raise, is the danger of auto-suggestion—that the dream will somehow fulfil the expectation of the experimenter. The objection really holds only against the most casual and desultory forms of experimentation. With regard to the methods here used, at any rate, we shall be able to show, from the nature of the results themselves, that auto-suggestion cannot be a factor in producing them.

(3) Writers have confined themselves mainly to the manifest content of the dream, to associations which are proved (or conjectured) to be related to that content, and in recent years under the influence of Freud to investigating the deep-level reactions—an inoffensive expression which we may use for the present—which can be deduced therefrom. But this manifest content, which can be described in narrative form and analysed as a narrative, is only one out of many features of the dream. Apart from the mere content as represented in words, dreams vary in endless ways. They are sluggish or swift, vivid or vague, smoothly flowing or broken and sporadic, and so on indefinitely according to the mode of comparison which we choose to apply. We are within the mark in saying that these essential aspects have not so far been seriously reckoned with. For instance, there is the all-important question of the lower limits of the dream. Everyone has observed that, beyond the dreams we remember, there are numbers which are only perceived as it were in the act of eluding the waking consciousness. The position really is, that there is no evidence that sleep is ever 'chemically free' from dreaming, while there are abundant indications that the lower limits of dreaming lie at any rate a good

---

[1] "Contribution to the Psychology of Visual Dreams," *Mind*, April, 1892, p. 299.

[2] *Über den Traum*. Ed. O. Klemm. (Leipzig, 2 vols, 1910–12.)

way beyond the power of the waking consciousness to effect recovery. A very little systematic observation will convince anybody that the vividness of dreams varies considerably, down to the point at which the chances of remembering or not remembering a dream are so evenly balanced that a grain will turn the scale. This vividness, although to read most books on dreams one would suppose it to be a constant, not only varies from one night to another, but can be varied artificially or by disease. In Prof. Freud's neurotic patients it is evidently high, as it is also under the influence of opium and other drugs. In a certain muscular, phlegmatic type of person, it is correspondingly low, which is possibly the reason why some people of that type assert that they never dream.

Besides the critical point of vividness, beyond which the dream tends to elude memory, there is a critical point of recovery which depends on other conditions. Morning headache, also a certain very common *torpid* state on waking, obviously make the process of remembering the dream confused and difficult, although it by no means follows that there has been no dreaming: and many other conditions and habits of life tend to banish the memory of the night's dreaming. In order to recover as much dream material as possible, there are at least two precautions which ought to be observed. (1) The awakening should be prearranged to be as gradual as possible. The loud ringing of an alarm, or knocking at a door, will make havoc of any dream which is in progress at the time. (2) On awakening, and before rising, the experimenter should be careful to impress upon his memory, in what is to be a fixed and permanent verbal form, the manifest content of the night's dreaming, so far as he remembers it: the written record, a simple transcript, to be made as soon thereafter as practicable. This gives us the *dream record*, which, if efficiently taken, has an absolute scientific value, independent of the operations of analysis and reasoning which may afterwards be performed upon it. Loss, uncertainty and distortion are apt to follow unless this procedure is observed.

(4) On the question of general method in experimentation, to undertake haphazard experiments in the hope that something interesting will turn up was very well for a pioneer like Maury, but will not serve for the higher development of the subject. The sound practice is to avoid adventures, and wherever a point of dubiety arises in a dream, whether that dream is free or controlled, to submit the point to a series of experiments—never to the arbitration of a single experiment—with a view to getting a clear and decisive answer. Such a point arises in connection

with the following 'free' dream. The reader is requested to concentrate attention on the elements italicised, and as far as possible to ignore the rest. Half the trouble in this subject arises from the fact that people will insist on attempting what they call 'complete' analyses, when they are barely able to point with certainty to the causation of a single element.

"D.R." signifies the dream record, as above defined, or the full night's dreaming (pure content only) so far as recovered. The Roman numerals indicate the successive movements of the night's dreaming in their true time order: generally speaking, only one movement is recovered, and this is what is commonly but not very precisely called *the dream*, although it is quite possible that the actual dreaming may have lasted through the entire night. Where the symbol (y) occurs, the meaning is that the element in question is traceable to the experience of the day before. Its main point is to avoid an occasional ambiguity which may present itself to the reader: it may refer to conversation, reading, things seen, heard or felt, or to any other mode of experience[1].

D.R. 1. Uncontrolled—*i.e.* no experimental element.

[Movement I. Find myself in a *crowded* railway carriage, and get off, for comfort's sake, at an intermediate station. On the platform the crowd is *denser* than in the carriage, and everybody is talking energetically about a topic of the day (y).

II. It becomes known to me that the train is divided into Legal, Medical and General Sections (y = name of an Insurance Company). Decide to continue the journey with the medical men. On my way through the *jostling crowd* I am hailed by an artist friend, whose face I distinctly recognise. Make towards the artist, but lose sight of him in the crowd.

III. Arrive at the medical part of the train, but find it badly crowded also, the passengers, as in Movement I, being 'jammed' tight, *shoulder to shoulder*. The train is about to start. I hasten back to my former carriage.

IV. Just as I near the carriage, the train gets into motion. I *shoulder* my way through the crowd, and seize with my *right* hand the brass rail of the last carriage. The door is locked, and I am left outside on the footboard.

V. Immediately outside of the station there is a tunnel of very narrow aperture, and to avoid being crushed against the walls I hoist myself, just before the train enters the tunnel, into a recumbent position along the upper outside edge of the carriage. But there is very little room even so, and my *right shoulder* is badly scraped by the tunnel wall.

VI. I become aware of the engine (apparently at the wrong end of the train), and of the engineers talking. I cannot remember the exact words, but they appear to have seen me. After a short discussion the train is stopped, and I clamber down. In doing so I slip, and fall heavily on my *right shoulder*.. At this point I awake.]

On rising I was conscious of some uneasiness in this shoulder, and on examining the place found a trifling pink bruise about the size of a shilling. The cause was a small fold or crease in the undersheet, at the place where the shoulder had rested. It is important to notice the exact dimensions of the fold—they were ½″ × ⅛″ broad × something under a sixteenth of an inch in height, or roundly, 13 × 3 × 1 mm. This was the cause of the *crescendo* of troubles in the dream. It will be said that this point was reached long ago by Maury and others. Not altogether so. By good fortune the intensity or vividness of this dream was high, and the other conditions good, so that I was able to remember right back to the probable starting-point of the dream. It will be observed that the dream proceeds from a *pianissimo* of light pressure through pushing, jostling, shoulder-strain and skin-scraping to the *fortissimo* of a violent bruise. It is an error in method, however, to lay any stress upon a single dream. I have given this one in detail in order to show that the series of experiments now to be described (Series I) was not undertaken upon any random hypothesis, but in order to clear up a definite question, suggested not so much by the later as by the earlier movements of the dream cited.

---

[1] The italics indicate the particular elements, the causation of which we are to attempt to trace.

II. Series I. Experiments with Tensors.

(5) If a tiny crease measuring 13 × 3 × 1 mm. can have these formidable effects upon the dream, it follows that we ought to get more ordinary effects by much feebler means. What is wrong with Maury's experiments is that they overshoot the mark, and instead of telling us anything about ordinary dreaming they introduce to our notice a special type, which may be termed the 'Maury dream.' What we want is approximately normal dreams, and if we have to get them by an artifice, it must be such as to stand fairly in lieu of such conditions as may occur in an average night's sleeping.

To meet this condition, one quite satisfactory method is to take a small piece of gummed paper, 2 cm. square, or slightly less, and to apply it to any selected part of the body. The difference made by the very slight 'tension' so introduced is not greater than can occur in many ways during sleep: from the contact of bedclothes or sleeping apparel; from the positions, attitudes and pressures of the sleeper; from the tensions, whether arising during the night or carried forward as residues of the day's occupations, which may be present in any part of the bodily surface. By the use of 'tensors,' a convenient term which requires no further explanation, we can make a complete survey of the body surface for its tension associations: for, as we shall see, the dream is by far the most delicate instrument we possess for searching out such associations. But such a survey would occupy a considerable time—not less than ten years. It would be absurd to begin on such a scale, and the present series is based on a rough division of the body surface into three-inch squares—the square being brought into action by applying one tensor to its centre. This has been sufficient, as the examples will show, to bring out with some fullness the part ordinarily played by small tensions in the mechanism of the dream. To assist those who may wish to test the matter for them selves, the following observations may be made:

(1) Except where the recovery is bad, which under the best conditions may be expected to occur in 5 per cent. of cases, the effect of the tensor will invariably appear in the dream.

(2) The writer uses for choice gummed paper as above described, but for particular purposes he has used sticking plaster, liquid skin plaster, gum, wafers, etc. to which list it would be easy to add. A separate question arises in connexion with variations of the type of tensor, into which I have not entered. The purpose in this particular series was to keep as closely as possible to the minimum tensor.

(3) By some the 2 cm. gummed paper tensor will no doubt be found inadequate as the basic minimum: the experimenter must bear this in view, and increase cautiously until his requirements are met. Conversely for others (especially for women with their greater sensitivity) a smaller tensor may be sufficient. In the writer's experience, a tensor of 1 × 1 or even 2 × 1 cm. produces, unless applied on exceptional sites, no appreciable effect.

There are numerous other particulars which will strike the reader if he attempts an experimental series for himself. The main point to keep in view for the present is that the tension induced is very slight, and that the results show conclusively that tensions beyond the reach of our waking perceptions may have a dominating influence upon the dream.

(6) The following examples are chosen mainly from experiments upon the limbs. Tensor experiments upon almost all parts of the trunk and head, while in other respects strictly in line with those on the limbs, are apt to be attended by affects, or something which closely approaches to the affects. One or two examples in this paper will show what is meant, and will indicate why it is desirable to keep the study of the tensor effect free for the present from those more difficult questions.

Examples of tensor effect.

D.R. 2. Tensor on mid-sole of right foot.

[People are *dancing* on a verandah, which also resembles a lighted stage. I am watching from a little way off, as if I were a spectator in a theatre. A woman, sort of heroine of the piece, dances in the centre, but *clumsily*, as if there were lameness in one foot or the other (undetermined). The other men and women are clumsy also, and suggest the effect of rustics dancing in a barn.]

Note. Saw Russian Ballet (2*y*). In other examples I shall only occasionally draw attention to the reminiscent material.

D.R. 3. Tensor on right mid knee-cap.

[A. known to be sleeping in an adjoining room, is heard to be speaking reproachfully and excitedly to intruders (burglars). Go to the room, and find A. sitting up in bed, *with knees flexed.* A. reproaches me for delay in coming: I accuse A. of having dreamt the incident.]

Note. Everything printed in small type within brackets is dream. Later movements are in general omitted. All show tensor effect.

D.R. 4. Tensor on left elbow, just below the point.

[Visit B.'s house, where I have not been for years. There are many visitors. B. is bent on showing us some object or feature of the house (not clear). I am looking into a room from a *crowded* doorway.]

Note. Pressure on left elbow. Cf. D.R. 1, crowding also.

D.R. 5. Tensor 1½ ins. below right nipple.

[Feelings of illness and anxiety. A doctor comes and applies his stethoscope to the *locus*. He then takes a bottle of turpentine, and rubs my chest. Under the supposed irritation of the turpentine, I slowly awake.]

Note. Observe the emotional component, which is independent of real pain, for there was none. The dream illustrates one of the many complications which may arise in trunk experiments.

D.R. 6. Middle of left shoulder-blade.

[Confused dream of sweeping with a long-handled broom.]

Note. The point in this, which appears more clearly in other examples, is that in the act in question there is special tension at the *locus*.

D.R. 7. Left lower leg, outer side, 6 ins. down from middle of knee-joint.

[Scene—a country road. Find myself caught between runaway horse and cart and a wall. Actual or threatened kick (undetermined) from the horse on the *locus*.]

D.R. 8. Tensor on right large toe, immediately next the nail, that is, over the upper toe-joint.

[I. It is late in the night; I am in an office building of many high stairs (known) and climbing them to get to a certain office.]

Note. Stair dreams are attributed by some writers to underlying sexual reactions. Here the main effect is undoubtedly, and the whole effect almost certainly, due to the tensor. In stair-climbing there is a maximum tension at the *locus* in this case.

[II. I enter a known office, and 'tiptoe' about, being afraid of being discovered as an intruder. I am aware that it is night, and am looking for something—vague. Perhaps I should add that my intentions are not burglarious.]

Note. There is a slight affective element (anxious caution) in the dream, which we may be justified in treating as inseparably associated with the act of 'tiptoeing.'

D.R. 9. Hollow under left knee, back of leg.

[Of this dream, which deals with known persons, it is sufficient to say that I was visiting a friend's house, chatting with various members of the family, and in a *sitting* position throughout.]

D.R. 10. Hollow or crook of right elbow.

[Back in an old house where I have not lived for many years. I am in my old room, playing a piece which was familiar to me then.]

Note. Observe that in the position for piano-playing there is tension at the crook of the elbows.

D.R. 11. Tensor on right mid-instep.

[I am in the company of a foreign visitor. We are going towards the E. hotel, which lies at the top of a sloping street. We are walking with our feet *against the slope.*]

D.R. 12. Knuckle of middle finger, left hand.

[Back in F. school, which had a class in wood-carving. I am standing in front of a bench with a chisel *grasped for use* in my left hand.]

Note. Another remote memory, attributable to the tensor alone.

D.R. 13. Tensor on right thigh—outer side—midway between hip and knee.

[I am looking at a photograph of the H. family. They are sitting on two parallel benches, with their faces and bodies *turned to the side.*]

Note. That is to say, they are sitting with their right thighs resting angularly on the edge of the bench, with maximum pressure at or near the *locus.*

D.R. 14. Tensor in middle of forehead.

[There has been a serious accident at a timber factory. High pile of timber falls, killing (60) workmen. There are two distinct falls—one of which I witness, and another is repeated in the presence of an architect called as an expert. Sense of disaster.]

Note. This is a facial experiment, and here again there is a real affect, of the kind which is associated with *strained* brow. But there is also a simpler, non-affective association, of wrinkled brow with looking upward. The reminiscence here was of a disaster to a very high football grandstand, of which I visited the scene.

D.R. 15. Left Achilles Tendon—just above line of ankle.

[I walk across fields to a broken bridge over a disused canal, and clamber over the gap in the bridge.]

Note. This is the revival of an actual incident, which occurred some years ago. The wooden bridge had lost some planks, and I had to manœuvre myself across as best I could. The hand support was on my left, and I had to advance the right foot a considerable way, throwing a severe strain on the left tendon.

In view of the fact that the reader, if he gives the method a trial, will get results of the same order for himself, it is needless to prolong the list. I conclude with a trunk dream which shows both tension and supervening *affect* very clearly.

D.R. 16. Tensor on the highest vertebra which can be distinctly felt with the fingers, just above the shoulder line.

[I. In H. grounds. An airship is high overhead. Head and eyes turned upwards on the airship throughout this movement. The airship is further confused with a factory or workshop. It is stationary, and many ropes dangle to the ground. Vague straining upon these ropes, by self and others, with heads bent backwards.

II. (Uncertain interval.) Go to a small shop to buy something—vague. See two jute bags containing a white powder, and decide to buy some. Conversation with somewhat unmannerly assistant. She says "Two shillings," omitting the conventional 'please.' Answer, "I'll buy it if you say please." Just then a sturdy middle-aged democrat appears in the doorway and interjects, "I wouldn't say please." I go out *haughtily*.

III. Vague ideas of walking through *tall* bare streets, full of factories (neighbourhood recognised).

IV. Public dinner of a company connected (as landlords, it seems) with the shops and streets. Speech by chairman: a speech of ordinary type, describing the properties owned by the company, etc. The speaker is white-moustached, elderly, and rather a 'fine gentleman': *stiff-necked*, or poker-backed, as a Victorian playwright expresses it.]

Note. The affect in II and IV is unmistakable, and is clearly associated with the dignity or emphatic 'self-respect' which goes with a head well flung back.

(7) The writer has made some 200 experiments (including duplicates) in this rough survey. The proportion of bad recoveries—about 5 per cent.—was satisfactorily small: on the other hand, considerable precautions were taken against disturbance during the critical phase of waking. Wherever clear recovery is effected, the tensor action appears unmistakably in the dream: not as an added element, but as of the essence or core of the dream situation. Duplicates do not, of course, produce identical dreams. They are worked up on the basis of varying states of the sleeper from day to day, and consequently of varying reminiscent material and other factors. Thus, tensor on centre of left knee-cap for both dreams:

D.R. 17.

[I am walking in K. factory grounds, which are traversed by service-railway embankments. I walk along one of these, my left side towards the sharp, cliff-like edge, and am conscious of a danger of putting the left leg over the side, so that I move that leg stiffly and carefully.]

D.R. 18.

[In this dream which was long and consisted of five movements, I was in an erect posture throughout, but sometimes standing, sometimes walking about. In one movement I was standing at a high desk writing, in which case there is special tension of the left knee: in the others there was only a faint distinction between left and right.]

In the last dream, it should be added, there were powerful (uncontrolled) factors present besides the tensor. It is conceivable that in some cases the co-present factors, without being strictly abnormal, may be strong enough to swamp the tensor effect. This occurs normally when the tensor is too small, but should occur very rarely when it is chosen of a proper size and strength.

(8) Those who undertake a systematic trial and investigation of the foregoing *type of effect* will find that the following inferences are warranted, not only by the above examples, but by their own experience.

(A) *Association and Freud's Hypotheses.* Simple association, open and direct, has on this showing a much larger part in dream formation than Freud allows. Considering the obvious frequency of slight tensions during sleep, their proved effects upon the dream, and the fact that he makes no provision for eliminating these, there is scarcely an analysis of his that can be taken for psychological purposes at its face value. The tensor method does not pretend to give complete analyses. It proposes only to trace the operation upon the dream of an accurately known factor—a very different thing from deducing the existence of unknown and uncontrolled processes by a system founded upon numerous *ad hoc* hypotheses.

(B) *Ocularisation.* What will perhaps strike the observer most is the preponderance of what may be termed 'ocularisation' in thought. The tensor is mainly operative as a tactile element, though it also has muscular effects, but this element enters the manifest dream chiefly by virtue of its ocular associations. In this limited sense at least it is true, as Freud constantly argues, that the less manifestly conscious may have the stronger operative force upon the dream; the tactile, although it is almost hidden from view, is really more important than the visual element, wherever there is strong surface tension. In most tensor experiments it is only when we examine the dream closely that we can trace the manifest ocular elements back to their true origin. Again, with regard to the retinal blotches of which so much has been made, it appears that they can have only a minor influence. At any rate, they exhibit a surprising plasticity, if we are to judge from the freedom with which they adapt themselves to all sorts of ocular associations based upon tactile stimuli.

(C) *Empathy.* It will be seen that the tensor effect operates upon the characters or persons of the dream in particular ways. The agent (or patient) may be (*a*) the dreamer himself, (*b*) another person, (*c*) a number of persons together. How are we to account for (*b*) and (*c*)? It seems to the present writer that there is only one existing explanation which quite meets the case. The doctrine of 'empathy'—that is, that in looking at a picture for example which represents human beings in certain attitudes we have faint but complete replicas of those attitudes within us—has been mainly applied in aesthetics, and perhaps not always prudently. The tensor experiments would seem to indicate that 'empathy' really is more or less present when we observe actions by other people: that we have not only cerebral 'ideas,' but sensori-motor extensions parallel with those of the person seen. It is not easy to understand why, in the dream, action by another is equivalent to action by self, unless we are habituated, as the 'empathists' contend, to have a faint sensori-motor replica of actions seen. If the doctrine of empathy is accepted, we derive two benefits: firstly, we reduce the transference of actions to a simple case of association, and secondly, we clear up the Freudian idea that when another person appears alone in the dream, that person is really the Ego; the other person retains his proper identity, with the exception that his conduct is so far guided as an empathic projection of something really in ourselves.

A point which tells in favour of empathy is this. In reviewing the dreams recorded, alike in this series and in others, I find that, wherever the agent is other than myself, the action is one which I am unable, or unaccustomed to perform, or am debarred by the dream situation from performing. Thus in D.R. 1 shoulder pressure implies a crowd: in D.R. 2 other people dance—I no longer do so myself, and can enjoy it only as a spectacle: in D.R. 3 there was some sound suggestive of alarm, but it is someone else who utters the alarmed cry, and later assumes the alarmed attitude. We can travel down the entire series with the same result. This principle is therefore well supported by fact, and will be found much more intelligible in theory than the doctrine that other people in the dream *are* the Ego.

(D) *Left and Right.* So far as the writer's experiments have gone—on this point they are not complete—they indicate that where there is empathy in the dream we see *mirror-wise.* That is, supposing the real tension to be in the right arm, in the dream it appears in the left arm of a person facing us, in the right arm of one whose back is turned to us. This appears to be the general rule: there are niceties into which we need not enter here.

(E) *Magnification.* For present purposes we may be content to accept Bergson's idea of varying planes of psychic intensity. The plane of sleep is evidently low, and therefore small external stimuli induce internal movements which are relatively great when compared with the existing general intensity. Cf. D.R. 1, where a slight irritation is equated to a violent fall. For ordinary tensions the stimulus is much less, and therefore the dream equivalent proportionately modified; the experiments, however, bring out clearly the fact that magnification always occurs.

(F) *Affects.* There is nothing surprising in the fact that an affect should be inseparably associated with a particular bodily strain. The reason why appreciable affects are so rare in *normal* dreams appears to be that the operative tensions—which, in spite of Freud's theories, are obviously one great part of the dream mechanism—are mainly in the limbs, of which the affective capacity is small. (But cf. the 'tiptoe' dream, D.R. 8.) Thus, while it is a well-established doctrine that affects do not arise from the mere dream-situation, no matter how impressive in appearance, this doctrine must not be made to apply in its full extent to cases where the dream-situation includes an actual surface tension as one of its intrinsic elements. Still less does it apply where there are tensions in the trunk or face.

(G) *Tension.* The exact meaning of this word can be discussed more conveniently after we have studied the 'detensors.'

III. SERIES II. EXPERIMENTS WITH DETENSORS.

(9) If a slight increase of local tension has a modifying effect upon the dream, the question at once suggests itself whether a slight decrease might not also have its effect. For ordinary experience shows that bodily states tend towards a mean, and it is deviations from this mean that excite what is called attention and so the mental processes associated therewith. Our common distinctions of stimuli as strong and weak, hard and soft, rough and smooth, and many others, are based just as much upon decrease as upon increase of an implied normal tension. Hence, there is no *a priori* reason to conclude, as one might hurriedly do, that the effect of reducing tension would be simply a blank sheet with regard to the formation of the dream. But the only way to settle the question is to make the experiment. A series of over 50 was devoted to this point, and the results are subjoined.

The main technical difficulty is to settle what we are to use as a 'detensor': a term which may be excused as the correlative of what precedes. We might soften the parts with hot water, or use some suitable mild chemical agent, if such can be found. But most of the possible alternatives introduce an element of confusion: for instance, hot water raises the important but very distinct question of the effect of change of local temperature. The writer has been content to adopt a compromise method, which does not yield results so precise as in Series I, but results which are sufficient to dispose of the question at issue. The detensor used is any safe oily matter: thick oil, glycerine, mild ointments, fresh or cream butter. The last mentioned is perhaps as effective as any if properly used. This is gently but firmly rubbed in over a circular space of about 3 inches diameter; less will not, in the writer's experience, suffice. It helps if the substance is very slightly heated before application, but this is not necessary.

(10) Naturally, one of the first trials made was with the palms of the hands, and from it there was obtained this somewhat surprising result:

D.R. 19. Detensor on Palms of Hands.

[I am in a business office in L. Street, I am sleeping on a sort of elevated couch in a corner. Business friends (recognised) are also in the room talking amongst themselves. They seem unaware of or indifferent to my presence.]

Note. This dream points to one of the things which we are to expect to encounter in detensor experiments.

D.R. 20. Detensor on back of neck. (*Locus* as in D.R. 16.)

[I see a large brown rat running along the floor. Plant my foot on its tail, and press down upon the rat with a stick, at the same time calling to N. to bring me a pair of tongs and a basin of water to drown it.]

Note. This is based upon an actual memory. The head and body are bent slightly forward, the eyes looking vertically downwards. This dream is in some respects the exact reverse of D.R. 16, a thing which often happens in tensor and detensor experiments on the same *locus*. The point to notice particularly, however, is the contrast between the energy of this and the lethargy of the preceding. To make intelligible what follows, we may anticipate so far as to say that in a detensor dream the effect is either (1) general languor, or (2) action with minimum tension at the *locus*. The second is the case here. See below (11) B (2), where an attempt is made towards an explanation.

D.R. 21. Detensor on mid-soles of both feet.

[Going upstairs or rather find myself on the stairs leading to a set of business offices, P. Street. At the corner of the staircase present in the dream the steps are broken all round, leaving an impassable gap, and I have to make the passage by *sidling* along a rail on my stomach with feet suspended in air.]

Note. A not uncommon type among uncontrolled dreams. It looks as if it were half way to the still better known 'flying' dream. Personally, the writer has no doubt that a flying dream could be experimentally induced, could we hit upon the right combination.

D.R. 22. Detensor on both knee-caps.

[Lying in bed, early morning. I am told that a charwoman is coming to scrub out the premises. I refuse to believe that she will come, and do not get up.]

Note. This of course was a dream, although there is nothing to distinguish the apparent narrative from that of a real incident. Here again is a dream of lethargic type, although, as in D.R. 19, there is contrasted activity in others.

D.R. 23. Detensor (2 ins. diam.) on back of wrists.

[I am holding two square boards, like bricklayer's mortar-boards, by the handles. My hands are turned vertically (*i.e.* as in grasping a straight stick held perpendicularly) and there is a lump of clay between the boards, which I am shaping, by a gentle rotation of the boards, into the form of a sphere.]

Note. This is a good example of the way in which the dream crystallises round a particular state of tension. The idea of a brown clay-like figure is traceable to yesterday. Between this and the sidewards position of the hands, which is the position of minimum tension for the experimental *locus*, the dream takes its form.

D.R. 24. Detensors on outer curved angles of shoulders.

[Fusion of two novels, (1) Stevenson's *Master of Ballantrae*, (2) a weak imitation read (*y*), recalling Stevenson. Many analogies in the dream. Family quarrel, as in the story. Duel, but in a long, narrow hall. I am with P., he and I being onlookers or seconds to the duellists. I am carrying a bundle or parcel under the left arm, pressed against the body. So also is P. The parcel turns out to be a Cardigan jacket, which one of the duellists puts on in place of his own.]

Note. In 'holding a parcel' we have a point of minimum tension at the *locus*, as can be seen by bringing the left hand round to touch the right side. Later, the association seems to be that a Cardigan jacket, being woollen, allows a greater freedom, decrease of tension, to the shoulders.]

D.R. 25. Detensor on Soles of Feet.

[Q., an obnoxious politician, is visiting the town of R. He is captured by the opposite side, bound, *put in a long box*, and sent back by parcel post to his native place.]

Note. Another attempt to get a flying dream, but even less successful than the first. It is the detensor, however, which determines what shall be done with Q.

D.R. 26. Detensor, right wrist, front or palmar surface.

[I. Fitting an extra tube into a pipe to divert an overflow of water from a cistern.]

Note. (*y*), except that the motion of the hand is vertical, as in D.R. 23.

[II. In my room at N. Street, putting a certain book back in its place on the shelf.]

[III. A vague dream in which S. is seen riding a motor bicycle somewhere in Canada.]

Note. In all three movements the hands are held vertically sideways, or turned so that the thumb is up and the little finger down. In III the handle-bars are turned vertically downwards. Comparing this with No. 23, we see that this position gives minimum tension for both front and back of wrist.

D.R. 27. Detensor on the buttocks.

[I. At a football match which is also confused with a theatre. I am directed to the wrong seat, and when I find mine someone else is in possession.]

[II. The next clear element in the dream is that there is an interval, and I meet with some friends. On coming back to look for my seat things are worse and I cannot find an empty seat at all.]

Note. We have here an interesting conflict between two factors, (1) a sense of local languor, (2) the incompatibility of this with the other result of detension, the absence of the feeling of pressure. In waking life we should be driven to choose by the compulsion of fact: but in the dream this compulsion is not present. No doubt many frustration dreams are due to similar causes.

D.R. 28. Detensor behind the knees in the hollow spaces between the tendons, as in No. 9 for the left knee.

[Tramway line in an American town. I am waiting for a tramcar. It is a 'two-decker' and I mount to the top. As I reach the top the whole car seems to extend upward, that is, the floor on which I am standing rises to a perilous height. I am standing in the front of the car looking down on the street, which is a great way below. Fear of accident, *i.e.* that the whole structure will fall as the car rushes forward, and I with it.] See note on next example.

D.R. 29. As in last dream.

[I am in a high railway signal-box. I pull the wrong signals. An express train passes, bringing down in some uncomprehended way a row of tall signals and telegraph poles.]

Note. In both dreams there are extraneous elements leading to grotesque combinations. The fundamental idea in both is that of falling. The thing that falls in the second dream is inanimate, and is what some writers would term a symbol: but that is a word of dangerously mystical associations, and I prefer to regard such questions as falling under the generalised principle of empathy. The reason why detension at this *locus* leads (under certain conditions) to dreams of falling or incipient falling is not difficult to understand. In standing on *terra firma*, a good part of the downward pressure of the body passes directly through this region; the removal of this suggestion of superincumbent weight naturally introduces the idea that one is falling. Compare the sensations at the knees which accompany height-dizziness. It is not suggested that this is the only source of falling dreams.

It is needless to cite further examples, since the object here is merely to illustrate some of the commoner modes of operation of detensors. We may fitly conclude Series I and II with a dream which shows both effects together.

D.R. 30. Tensor on right palm, detensor on left palm.

[T. shows me a new dress which has just come from the maker. I hold it slantwise in front of me, right hand up bearing the weight, the lower part of the dress resting on a chair, the left hand touching the material lightly, palm open.]

The result of about 100 detensor experiments, some belonging to this and some to more specialised series, is that the detensor effects are quite as important for the mechanism of the dream as those studied in Series I. They are less homogeneous in one direction for a reason which will appear presently, but they are equally pronounced.

(11) If we review the detensor (and tensor) experiments, the following remarks seem to go no further than the plain indications.

(A) *Tension.* What in the light of Series I and II are we to understand by the word 'tension' as applied to the body surface? The word itself is perhaps not the best that could be used, but it is so far justified by this consideration, that in modern languages, apart from its usage in pure dynamics, it has the general meaning of 'tightness' as opposed to relaxation, much more than that of simple extension as opposed to contraction. It is in the former sense that the word is used here. The body surface is a more complex structure than any known in dynamics, and in it a perfectly simple stretching tension scarcely occurs at all. What we get is three principal forms (there are many variants), as follows:

(1) Inward tension, or pressure together of the tissues, more or less present in all dreams involving bodily movement.

(2) Passive tension, either oblique or normal to the surface, as in dreams of crushing, grazing, bruising, also in dreams of pressures arising from position, as sitting, leaning, etc.

(3) Outward tension, the least common, seen for example, in D.R. 3, in which the skin is really stretched 'tight as a drum' over the knee. This, which comes nearest to the simple mechanical idea of tension, occurs mainly in strained rigid attitudes, which naturally are rare in actual life.

If we remember further that in all cases there is pressure by and upon adjacent tissues, we realise that a clean-cut mechanical definition is out of the question. But if it is difficult to find a quite satisfactory definition of surface tension in the body, it is enough for practical purposes to take it as having the general meaning of tightness or strain.

(B) *Detension.* Conversely, detension is best defined, not in strictly mechanical terms, but as a relaxation or loosening. But here a distinction comes into view. In Series I we always found action of or upon the *locus*. In Series II we find either general inaction, which is easily understood, or some action, which may even be very energetic, in which the *locus* is in some way involved. The apparent inconsistency disappears on a closer examination. (1) The most obvious effect of a detensor is to induce local languor, which under certain circumstances (fatigue, etc.) assumes the form in the dream of general lethargy. (2) But this effect may be altogether superseded when the general tension is relatively high: for then the *locus* becomes simply a *point of minimum tension,* and the actions of the dream must conform themselves to this condition.

(C) *Relaxation during Sleep.* From the total results of Series II it would appear that the relaxation induced by the detensor falls considerably below the ordinary or average relaxation of sleep. The contrast between local and general languor appears in all the dreams: because in those (D.R. 19, 22) where general lethargy appears openly in the manifest content, there is still activity in the dream background. That is to say, the *locus* is still a point of minimum tension, and the other elements represent empathically the unsubmerged or free tensions which persist. If the general tension were uniformly reduced, we might, in theory, arrive at a stage in which the mentality of the sleeper corresponded exactly with his physical state. That is to say, there may occur at least an approach to a state of 'absolute sleep,' in which all cerebral activity is abolished. This state probably never comes near to realisation in people who use their brains to any extent: but it may be approached by muscular active people (labourers, sportsmen) who exert themselves into a state of physical lassitude during the day.

(D) *Affects.* Whether special affects may occur in connexion with particular detensor *loci* is a question which may be postponed for the present. The general affect which arises under suitable conditions is that of lethargy or languor. Conversely there is a more obscure general affect induced by tensors, namely, that of excitement or strain. This distinction between tensions and detensions, which, in practice, is sometimes sharp, sometimes faint, can only be brought out clearly by a special experimental series. From an alternating series of 25 on a selected *locus*, the palms of the hands, the general trend of affect unmistakably emerges. This series I am unfortunately unable for want of space to include in the present paper.

(E) *Auto-Suggestion.* Considering (1) that the tensor operates much or little or not at all according to its size and strength, (2) that tensor and detensor have diametrically opposite effects, and (3) that the anatomical results involve difficult and unforeseeable questions of maximum and minimum tension—considering those points only, and without entering into any elaborate argument, we may take it that the influence of auto-suggestion is here negligible. The writer may add that in the majority of cases it is only when he has risen and dressed, and placed himself in the position of the dream, that he is able to satisfy himself that this position really is one of maximum or minimum tension as the case may be.

IV. FUNDAMENTAL DATA IN DREAM STUDY.

(12) To say that there are many fundamental facts relating to the dream which are capable of experimental verification, is to put the matter as moderately as possible. It ought to be possible for students of the subject to agree upon a basis, however much they may differ

upon more advanced questions. In the hope of contributing to this desirable result, the writer ventures to set down the following data, which after a careful study of many hundreds of controlled dreams appear to him to be fundamental.

I. *Simple Modes of Dreaming.* No dream can ever be perfectly recorded in words, for reasons already indicated. The manifest content which we recover and are able to record is assumed of necessity to have been the most vivid part of the actual dream. Dream theory is mainly concentrated upon the causes which compel the manifest content or 'narrative' to assume the forms in which we actually find it. Where the form is obscure or highly complex we may concede that there is room for a large variety of explanations. But the study of controlled dreams brings to light certain elementary modes of dreaming, as to the causation of which, it appears to us, there need be no controversy. The simplest of these may be termed the Tension-Association (T.A.) type, of which the canal-bridge dream, No. 15 *supra*, may be taken as an example. In the causation of this type the subjective data are obviously (1) Perceptive magnification, (2) Associated concept of activity, including within itself this magnified perception, and involving the recall of a specific experience, and (3) Belief in the reality of this activity[1]. This is possibly the simplest of all modes of dreaming. The manifest content explains itself and needs no further interpretation.

II. *Residues and Cohesion.* If someone asks us, "What is it that is white, soft and cold?" we instantly answer, "Snow." In the same way, when the ideas of whiteness, softness and coldness are co-present in the dream, no matter from what sources they are severally derived, they cohere into the idea of snow. Take the idea of whiteness. Nothing is easier than to impose a stimulus of whiteness so powerful as to leave a real 'residue': it may be done by looking fixedly at a sheet of glossy white paper for a minute or two on end. This residual whiteness is not a memory, in the ordinary sense of the word: it is not conditionally but actually present in consciousness. It is doubtful if it ought to be called a memory at all, but if so we must distinguish a special class of memories, belonging to the day before and sometimes a little further back, to which we may apply the name of 'residues.' During the day these residues accumulate, but are in general of too low intensity to be part of the manifest consciousness. In the waking state the residues are submerged and suppressed: during sleep they reappear upon the surface, and are in fact one of the main groups of formative elements for the dream. The only conditions governing their emergence in the dream appear to be (1) the strength of the original stimulus, (2) the interval elapsing between the time of stimulus and the time of subsiding into sleep. A sufficiently powerful residue will influence, like a tensor, the whole night's dreaming. For less powerful residues there are strong indications of the presence of a time-law, according to which (other things being equal) the more recent enter the dream first.

---

[1] On 'belief,' cf. Prof. Ward, *Encycl. Brit.* "Psychology," pp. 597–8, and note that in the dream we are cut off from all correcting perceptions.

Similarly, the effect of the tension which is sufficient to modify the dream, is yet negligibly small for the waking consciousness. An ordinary gummed paper tensor, placed upon almost any part of the body, is lost to consciousness within a few minutes of being placed in position: yet we have seen what effects it can produce during sleep.

III. *The Tension-Residue Type.* By simple cohesion between the elements so described we get the next higher type, the Tension-Residue (T.R.) type as we may here term it. In this type also we have a tension-association, but of a slightly more complex kind. The tensor effect unites with the residue to revive the nearest associated conceptual activity which combines both. An example is the clay-ball dream, No. 23 *supra,* which shows the T.R. type in its simplest form. But this type is capable of a good deal of elaboration, as more numerous and more complex residues enter into it. Cf. the family photograph dream, No. 13, in which the name H. and a newspaper group picture, both residues, combine with the tensor effect in a complex conceptual activity.

The frequency of manifest contents of the T.A. and T.R. type is not easy to determine, since there are numerous intermediate cases where an obscure dream formation may be due to an untraced residue, or an unobserved additional tension. Exact statistics would therefore be misleading, and I can only say that in my experience the proportion is roughly about 10 per cent. of the total. With regard to neither of those modes of dreaming does it seem that any radically new principle is involved.

IV. *Dream Causation.* So far as experiment indicates, the primary or nuclear elements of the dream are normally tensions and residues. What we have is not specialised but reduced activity. The small tensions and residues which go to form the dream are inoperative during waking hours, because they and the associations which they arouse are submerged in the onrush of experience. In the quiescence of sleep, however, they have a free field before them, no competition, nothing to inhibit their operation. It is true that the cerebral activity is much reduced, but what remains is sufficient for the modest requirements of the dream.

V. *Classification of Tension Dreams.* There are three well-marked classes of Tension Dreams, if we consider the nature of the activity involved. These are (1) Habitual, where the revived action or position is in current use at the period of the dream, (2) Epochal, where it belongs to some period in the past life of the dreamer, (3) Unique, as in the canal-bridge dream, No. 15. The first class is by far the largest. The second is not uncommon in the writer's experience: incidentally, it is probably the type to which the dreams of Rosegger, the poet and ex-tailor (cited by Freud) belong—for here we have a vocational history which could not fail to be revived by suitable tensions during sleep. The third is comparatively rare, and seems to occur mainly in the pure T.A. state.

In the ordinary case there is an inrush of secondary associations as soon as the dream nucleus is formed: this is probably the main reason why manifest contents of the pure T.A. and T.R. type are in a decided minority. Those secondary associations may or may not appear in the manifest content, but in either case they modify its form more or less.

VI. *Material of the Dream.* All this helps to clear up a point which has given rise to much discussion among students of the subject, namely, why the more important events of the day so often fail to appear at all in the dream. The formative elements of the dream are, on the one hand, tensions arising during sleep, on the other residual tensions, sensory or sensori-motor residues, all products of the day's activity. There are others, but in normal dreams at least those mentioned decidedly preponderate. It is clear that an event may be 'ethically' important, and yet leave no appreciable sensory or other residue behind it. Or again an event (this is the common case) may be accompanied by a residue, which emerges in the dream divorced from its context. The matter really requires a much fuller discussion than can be given here, but there is one point to which attention should be drawn. Writers commonly assume that they have the whole night's dreaming before them, but that is rarely or never so. There is nothing to prove that an event of the day may not have appeared and disappeared in the early stages of the night's dreaming, where that dreaming has been imperfectly recovered.

VII. *General Operative Factors.* In this paper we have not considered states of the special sense-organs, nor states of the muscles, glands and viscera. The last mentioned group appear to act (otherwise than upon affect) in the direction of modifying the *tempo*, the continuity and other commonly unregarded features of the dream: although the 'rebirth' dream, which is a special case of broken continuity, has attracted a good deal of attention. It is not excluded, however, that these factors may operate either by direct or by affective association: in the case of dreams of visceral origin, particularly, they tend (in the writer's experience) to produce contents of the infantile-epochal type. We have not considered variations of lighting (dawn, moonlight, position of sleep, right or left eye on pillow, etc.), apparently unimportant elements which have a decisive influence upon the form of the dream-picture: nor many other variations of objective origin which may modify the form of the dream. When we take into account that all the factors mentioned may operate separately or together, we realise that there is room for an enormous variety in the structure of the dream,—a variety which can be studied without going beyond the safe limits of experimental control.

VIII. *Abnormal Dreams.* The writer must guard himself against the charge of disputing the value of dreams as an aid to diagnosis. The idea of a symptomatology of dreams is an old and a very reasonable one. To speak without prejudice to any present-day theory or method of treatment, there is one solid and indisputable fact which underlies the whole controversy: namely, that it is as an efficient branch of symptomatology that the study of dreams must make its permanent appeal, if at all, to the medical profession. The presence of psychic or other trouble means, it would be absurd to deny, the presence of a positive disturbing factor, the operation of which may dominate the form of the dream. In those cases where a pronounced disturbing factor is known to exist, medical observation functions in much the same way as experimental control. For normal dreams, on the other hand, an uncontrolled analysis is too often a purely speculative analysis, as it is also for morbid dreams in certain contingencies. An example is the obvious danger of ascribing to a remote association elements which are really due to a neglected primary tension: there is reason to believe that this mistake is not uncommonly made. Here, then, it is as much to the interest of the medical as of the general psychologist to strengthen his position by making a thorough experimental survey of the ground.

SUMMARY.

(13) 1. The dream represents only a reduced—*i.e.* the minimal—activity of the brain: complete inactivity being regarded as inadmissible.

2. The development of the dream is governed by the configuration of tensions in the dreamer's body.

3. A dream is of doubtful evidential value unless it is controlled for surface tensions.

4. Nothing is said against the competent use of dreams as an aid to

diagnosis, where a positive disturbing factor is known or suspected to be present.

5. Doctrines based upon the abnormal ought only to be imported into normal psychology with the greatest precaution.

6. States of the sense organs (apart from the tactile) and of the muscles, glands and viscera have not been considered in this paper.

7. The operation of the sense organs, etc. upon the dream can be determined by experiment, without recourse to abnormal psychology.

8. It is reasonable to conclude that by these and other methods the study of the normal dream can be placed on a solid and non-speculative foundation.

(*Manuscript received* 29 *April*, 1922.)

FURTHER NOTE ON THE 'THEORY OF TWO FACTORS'

BY C. SPEARMAN.

IN the last number of this *Journal*, a brief response was made by me to some of the objections so interestingly and emphatically raised against this theory by Prof. G. Thomson. Moreover, most of his arguments have been conclusively answered by Udny Yule[1] and by Maxwell Garnett[2]. There remains, however, still one objection which has never been answered, and has even been widely assumed to be unanswerable. This objection was aimed against a corrective formula used by me to eliminate the influence of sampling errors upon the principal values at issue, namely, the correlations between the columns composing an ordinary square table of correlational coefficients[3].

Now, this objection was partly based upon my having treated as negligible a certain quantity [denoted by $S(\rho'\epsilon)$] whilst nevertheless taking into account another quantity [denoted by $S(\epsilon^2)$]. Thomson has urged that the two are really of the same order of magnitude. But hereby he omits to notice that the first of them tends randomly to positive and negative values which cancel each other *in the average of different tables* (on which alone all argument has hitherto been based), whereas the second quantity obviously remains always positive and therefore uncancelled.

Thomson's other ground of objection, however, is a remarkable one. The usage of the corrective formula had been restricted by me to those cases where the correction to be applied was of limited magnitude; the standard had been laid down, that the ratio of certain prescribed deductions to the quantities from which they had to be deducted should never exceed at most 1:2. Thomson, in common with many other writers, charges this limiting standard with being arbitrary. But to him alone appears to belong the credit of submitting the matter to detailed laborious investigation. He indicates the undeniable and indeed obvious fact that, if the said ratio of the deductions to the quantities from which they have to be deducted became much greater than the adopted limits of 1:2, then, by the structure of the formula, the total result of the correction would necessarily be enormously too large. Thence he concludes that even on stopping short of this limiting ratio, the average total result must still be greatly excessive. Save for such excess, he declares, the intercolumnar correlation would be no greater than ought to be expected from a merely random distribution of the elements constituting the correlated variables. But, he goes on to maintain, so extraordinarily excessive is the correction when it approaches anywhere near to the limiting standard, that it raises the average of all the corrected intercolumnar correlations derived from the whole table up to no less than complete unity; by whatever amount the cases involving the smaller corrections fall short of this unity, by just this amount the cases involving the larger corrections will go even beyond it.

---

[1] This *Journal*, 1921, XII, 100.

[2] *Brain*, 1921, XLIV, 332.

[3] *Essentials of Mental Measurement*, 1921, chs. IX and X. Thomson regrettably overlooks the fact that even the true intercolumnar correlation was not advanced by me as the strict criterion of the Theory of Two Factors, but only as a convenient approximative substitute.

As a pure hypothesis, this view seems to me deserving of high praise for its ingenuity and even boldness. But I am less able to concur with his procedure by way of obtaining evidence. Let us consider what is really requisite for such a purpose. Firstly, there should be a definite statistical theorem demonstrated, to show how large an intercolumnar correlation would in truth by the laws of probability result on an average from a merely random distribution of the elements. Further, a definite proof should be given that the said corrective formula does actually—if ratios of deduction be admitted up to the limit of 1:2—produce such extraordinary excesses as to make the total result in a large proportion of the cases far exceed even unity itself. And lastly, some explanation would seem due as to why this nice balance at just unity on an average should have been so continuously maintained throughout all the extremely varied conditions (including those with respect to the ratios mentioned) that have hitherto governed the data submitted to inquiry. Now it is surprising and disappointing to find that of all this indispensable evidence Thomson has so far not even attempted or suggested bringing forward any portion whatsoever.

In such default, the following brief contribution may be acceptable. The first required piece of evidence, indeed—that as to the intercolumnar correlation to be expected from mere chance—is reserved for a later communication. But evidence on the second heading, and by inference on the third also, has been obtained as follows. Let R_{ab} denote the correlation between any two columns, a and b, as actually observed and therefore as affected by an error of sampling. Then by definition,

$$R_{ab} \equiv \frac{S\,(r_{xa} - \bar{r}_{xa})\,(r_{xb} - \bar{r}_{xb})}{\sqrt{S\,(r_{xa} - \bar{r}_{xa})^2}\,\sqrt{S\,(r_{xb} - \bar{r}_{xb})^2}},$$

which may be written as $S_1/S_2^{\frac{1}{2}}S_3^{\frac{1}{2}}$,
where scoring signifies the means for all values of x. Putting ticks to signify values as purged of the sampling errors, we get at once by Taylor's expansion,

$$R_{ab} = R_{ab}' + \frac{\partial R_{ab}}{\partial S_1} dS_1 + \frac{\partial R_{ab}}{\partial S_2} dS_2 + \frac{\partial R_{ab}}{\partial S_3} dS_3 + L,$$

which works out easily to

$$(R_{ab} - R_{ab}')/R_{ab}' = dS_1/S_1 - \tfrac{1}{2}dS_2/S_2 - \tfrac{1}{2}dS_3/S_3 + L',$$

where L and L' involve the higher differentials. Clearly, each dS can be replaced by its average value in a large number of random samples. If, further, we neglect the L' involving the higher differentials—a proceeding accepted without demur in the greater part of the current statistical formulae—then we get by straightforward calculation a result not differing appreciably from my original corrective formula; the discrepancy between the two rarely exceeds 1–2 per cent.; and when S_2 and dS_2 are equal to S_3 and dS_3 (as they usually tend to be), the two formulae become identical. The above-mentioned limiting ratio of 1:2 applies directly to $dS_2 : S_2$ and to $dS_3 : S_3$, whilst indirectly it involves a corresponding limitation of $dS_1 : S_1$.

But the present mode of proof enables us to take into account also the terms involving the higher differentials, so that the correction can be brought as nearly as desired to perfect accuracy. These supplementary terms are easily calculated by any one; they form a series in dS_1/S_1, dS_2/S_2, and dS_3/S_3 of second and higher orders. They are accordingly negligible when, and only when, these ratios are small.

Now, I have taken the trouble to work out the preceding formula as far as the *third* differentials for a number of cases taken from the very data published some time ago with especial intent to *dis*prove the Theory of Two Factors, namely the publication of Simpson[1]. Here, the ratios of the required deductions to the quantities from which they have to be deducted happen to range from 1:8 to nearly 1:2, the average lying between 1:5 and 1:4. For any such average values, the excess resulting from the original formula (as judged by comparison with the more perfect formula including the higher differentials) turns out to be only about 1 per cent. Even for the lowest pair of ratios among all those that passed the standard (*i.e.* the intercolumnar correla-

[1] *Columbia University Contributions to Education, Teachers' College Series*, 1912, No. 53. The author himself, apparently, tried to estimate the bearings of his results by simple inspection of them. On my actually working them out, they proved just the opposite to what he had supposed (see *Psychol. Rev.* 1914, XXI, 101).

tion between the tests for Adding and Recognition), the results are as follows:

(*a*) Intercolumnar correlation actually observed	·74
(*b*) The same as corrected by the original formula	·92
(*c*) As corrected by the above formula going only up to first differentials	·92
(*d*) As corrected by the above formula up to third differentials	·88

Thus even in this extreme instance—quite swamped, of course, in the total average—the discrepancy amounts to no more than 4 per cent. And such a conclusion derived from our formulae agrees excellently with an empirical consideration of Simpson's data. For on dividing all the corrected intercolumnar correlations (23 in number) into two groups according to the magnitude of dS/S, the averages for the larger and the smaller corrections respectively come to ·96 and ·95—a discrepancy that in all previous discussion could quite well be neglected. On the whole, the above evidence indicates that the standard of 1:2 was chosen judiciously enough for a first rough survey of the facts, being intended to include the largest possible number of cases and yet not to suffer from any seriously perturbing excesses. For finer work, and especially for the ampler data more recently available, the standard might perhaps advantageously be stiffened up to 1:3, 1:4, or even 1:5. By so doing, as has just been shown, the general average for the whole table would be diminished to an extent not significant for most purposes, but still possibly of interest in special cases.

There still remains to be considered the evidence adduced by Thomson of an empirical nature. By a clever device he obtained correlations in duplicate, the one version being regardable as 'true,' the other as 'observed' and therefore affected by errors of sampling. To the 'observed' values he applied my (original) corrective formula in order to test its consonance with the 'truth.'

So far, excellent. And if the figures given are merely intended as illustrations, little more need be said on the matter. But if the figures are meant to have any evidential force, this we cannot for a moment concede. To attain to this, such deliberately manufactured data would at least have to be furnished on a very large scale (and obtained under far more rigorously defined conditions)[1]: for they claim to check formulae expressly referring to large averages only (whether or not the higher differentials are taken into account, see above proof), whereas Thomson only brings forward *one* arbitrarily selected set of correlations. These, too, are based upon only 20 individuals; they are therefore liable to extreme variations, of which his arbitrary selection can take advantage so as to produce any result he pleases. Further, even this single set of correlations is such a poor one for the present purposes as to afford him only five intercolumnar correlations claimed to be usable—a quite trivial number to stand alone as evidence. The distrust of these five values is but heightened by their displaying throughout, in spite of their exiguous basis, a high degree of regularity; this feature is, I am obliged to suggest, wholly unnatural. Indeed, even as regards the handling of the figures in detail, the reader's confidence is apt to be shaken. For instance, of the scanty five intercolumnar correlations thus brought forward as passing the standard ratio of 1:2, I have worked out one case for myself (that of x_2 and x_6) and find that even here the standard is really *not* reached. Similar misgivings must be extended to the most familiar mathematical operations; I have tried one of the five 'observed' intercolumnar correlations themselves (that between x_6 and x_7), and I make it out to be not ·63 as given, but approximately ·86[1].

---

[1] Compare the admirable work of this kind done by Thorndike and by "A Student."

All in all, the whole notion that the corrective formula hitherto used produces huge excesses in the resultant value—and herewith, of course, the further notion that such excesses somewhow or other always manage to generate between them an average of just unity—these notions appear never to have been supported by any serious evidence and to be now definitely disproved. The corrective formula, though doubtless defective in the sense of only applying to a portion of the data, shows no sign of defectiveness in the sense of being invalid with regard to this portion.

---

[1] The reader can immediately check this for himself. The one series has the values, 50, 45, 60, 63, 22, 29, −16, 18; the other, 72, 48, 43, 75, 32, 67, −26, 10 (p. 178).

(*Manuscript received* 28 *September*, 1922.)

BILINGUALISM AND MENTAL DEVELOPMENT.

By FRANK SMITH.

I. *The problem* (pp. 271–273).
II. *Preliminary experiments* (pp. 273–275).
III. *A two years' investigation* (pp. 275–282).
IV. *Conclusions* (p. 282).

I.

THE important part played by language in mental growth has been emphasized by every psychologist, and no educationist now questions the supreme claim of the vernacular to the first place in the curriculum, long and bitter though the controversy has been. No second language can be so intimate as the mother tongue, so bound up is it with the child's personal life, so overlaid and intensified by emotional currents, and so closely wedded to his intimate and commonplace experiences.

> Every word almost (as Laurie[1] says) is rich in unexpressed associations of life-experience, which gives it its full value for the life of mind. Subtleties, and delicacies, and refinements of feeling and perception are only indicated by words. The rest lies deep in our conscious or half-conscious life, and is the source of the tone and colour of language, and of its wide reaching relations with all that is not itself and yet is itself. Words accordingly must be steeped in lite to be living, and as we have not two lives but only one, so we can have only one language.

The problem of the bilingual child, who acquires two languages as the monoglot acquires one, is therefore of considerable interest to the psychologist, and of immediate importance to the educator. Yet it has attracted comparatively little notice. The most careful study of the development of a bilingual child is probably that of Jules Roujat[2], whose child was spoken to by his father exclusively in French and by his mother exclusively in German. Roujat kept a careful record of the boy's progress, and showed that the acquirement of two languages is as easy as the acquirement of one, that the two modes of pronunciation are independently organized, and equally efficient, and that the vocabulary and syntax of each language are kept apart with complete success. But Roujat threw no light, and probably he would not be justified in raising the question with such data as he collected, upon the mental effects of bilingualism in the case observed. The monograph, therefore, only answers one-half of Laurie's assertion[1] that "if it were possible for a child or boy to live in two languages at once equally well, so much the worse. His intellectual and spiritual growth would not thereby be doubled, but halved. Unity of mind and character would have great difficulty in asserting itself in such circumstances." The possibility cannot be doubted; but the effects of it are still to seek.

---

[1] *Language and Linguistic Method* Cambridge University Press, 1890.

[2] *Le développement du Langage chez un Enfant Bilingue.* Par Jules Roujat. Paris: Champion.

Now this problem, which is of practical importance, is usually raised and dismissed with as little hesitation by those who would accept Laurie's conclusion as by those who would deny it. In Wales the claim is frequently made that a bilingual education is one of the greatest blessings which circumstances have bestowed on a fortunate land, "affording the democracy," as one orator has said, "an intellectual training more or less equivalent to the classical studies which are in England the privilege of the affluent." We may dismiss such exaggeration as mere foolishness, but the same argument is put, though more carefully, by serious writers. Thus J. G. Williams[2] lays it down as an "axiom," accepted by the "enlightened conscience of educators," that to teach Welsh to English-speaking children in the elementary schools is an "intellectual advantage." Stanley Leathes[3] accepts the position: "I think that bilinguals, like the Welsh, whose education is carried on in two languages, must get more from their elementary schools than the scholars of a country like England, where only one language is used."

On the other hand, there are writers who deny the assertion as bluntly as it is made. J. V. Morgan[4] appeals to great names:

> The Rev. Thomas Jones, the celebrated poet preacher, friend of Browning, and father of two very eminent Welshmen—Sir David Brynmor Jones, M.P., and the late Principal Viriamu Jones—was greater in English than he ever was in Welsh, and that partly owing to the fact that he had broken with the vernacular. When asked why he was no longer as effective in Welsh as he used to be, he replied that he had come to the conclusion that he could not retain his power in the English pulpit if he continued to preach also in Welsh.

---

[1] *Language and Linguistic Method*, p. 15.

[2] *Mother-Tongue and Other Tongue.* Bangor: Jarvis and Foster, 1914.

[3] *What is Education?* London: Bell and Sons, 1913.

[4] *A Study in Nationality.* London: Chapman and Hall.

And perhaps more significant are the words of a great language teacher, the late Hardress O'Grady[1]:

> But are the psychologists quite sure that early bilingualism is an educational asset? I have heard powerful arguments against ambidexterity. And for a child learning to attach labels to objects, actions, learning to *think* by means of words, *is it really an advantage to have two words, three words, for the same thing?*... Of myself I can say that I have had great difficulties to overcome in matters of thought, speech, expression, and that I attribute these difficulties to early bilingualism and the constant struggle of two languages for precedence.

It is not necessary to examine these assertions closely, so obviously inconclusive are they. Psychological theory allows us to admit an "intellectual advantage" in the acquirement of a second language, but only in the same sense that the study of history, or geography, or science, is an intellectual advantage. All these subjects may make demands upon the highest mental activities, though, of course, the demands will not be of the same kind. Of a pupil who studies a language we cannot assert, as did educationists of old, that he is thereby made more mentally alert than one who studies a science. The mental gains differ in the two cases, and nobody has yet proved that either is superior. There is one aspect of this argument, too, which needs to be kept in mind, namely, the assumption that the study of a second language must, by comparison with the first, bestow concepts of its strength and weakness as a medium of thought, as a mode of expression, and as literary art. But how often is this result achieved in scholars whose education ceases after the elementary school is left?

II.

It was in the hope of testing the assertion that bilinguists are mentally superior to monoglots that the following experiments were carried out. They occupied, in all, more than three years. The problem, as it presented itself to me first, was to ascertain whether there was any marked superiority in the same school, where the two groups of bilinguists and monoglots worked side by side, in the same classes, and under the same conditions. It is not easy to find schools where both groups are found in anything like equal numbers: in Welsh rural areas the children are preponderatingly bilingual and in the large towns of South Wales they are chiefly monoglots. But with the aid of students I was able to carry out preliminary tests in five schools in four different parts of Wales, where the classes were fairly equally representative of the two groups. The scholars were classified by the teachers, and a few doubtful cases were excluded. Standards III and VII were chosen for the tests, so that the influence of the growth of four years might be revealed. The tests were chosen from Whipple's *Manual*, and consisted of:

---

[1] *Times Educational Supplement*, Jan. 18, 1917.

1. A passage of unprepared dictation.
2. A mutilated passage test.
   (This was written on the blackboard, and the scholars were required to write down the omitted words.)
3. A sentence-forming test.
   (Three words were given, as in Whipple.)
4. A word-forming test.
   (Six letters were given, as in Whipple.)
5. Free composition.
   (A theme was announced, and the scholars were allowed 15 minutes in which to write what they could. Two such essays were written by each child, one on a practical, matter-of-fact topic, the other on a more imaginative subject.)

Not all these tests were used throughout. The third proved too difficult for the younger classes, and the second test, as given by Whipple, ignores the objection that all omissions should be of nearly uniform difficulty. Dictation, too, is a questionable test of intelligence. I am inclined to think that the fifth test is the most satisfactory one, for in 15 minutes a scholar can show something of his ideas and his command of language, even if he is slow of response. But it is difficult to give the essays numerical marks, since the school standards vary. I divided them, therefore, into five classes only, and used the numerical marks 1 to 5. Thus, 3 stood for average ability at the age examined, 2 and 4 represented work somewhat below and above respectively, while 5 meant exceptional (and rare) ability, and 1 serious weakness. A scholar's work could thus be judged without hesitation. The results of the essay writing in the five schools are shown in the following table:

	STANDARD III		STANDARD VII	
School	Monoglots	Bilinguists	Monoglots	Bilinguists
1	4·5	3·7	7·4	5·2
2	4·8	4·0	7·7	6·6
3	5·6	5·8	8·4	7·9
4	5·4	4·6	6·9	6·0
5	4·7	4·8	7·5	7·0

Thus, in Standard III, the monoglots, on the whole, have an advantage over the bilinguists, and this advantage is very much more pronounced in Standard VII.

The word-forming test gave the following results:

	STANDARD III		STANDARD VII	
School	Monoglots	Bilinguists	Monoglots	Bilinguists
1	3·64*	3·79*	8·5	7·2
2	6·4	6·4	16·0	12·3
3	6·25	6·43	12·4	12·2
4	6·8	4·9	11·4	11·3
5	5·3	5·1	9·4	10·0

* Less time was allowed in this school.

In Standard III neither group can claim the advantage, but in Standard VII the monoglots show a slight superiority in four schools.

The other three tests I carried out in one school only, with the following results:

	STANDARD III		STANDARD VII	
	Monoglots	Bilinguists	Monoglots	Bilinguists
Test	%	%	%	%
Dictation test	54	55	50	39
Mutilated passage test	57	52	—	—
Sentence-forming test	19·3	16·1	50	43

Regarded as a whole, therefore, these tests indicate that the monoglot child has an advantage over the bilingual child, an advantage which increases during his school-life.

III.

Such an investigation could only be regarded, of course, as superficial. The outstanding weakness of it is that the groups compared are really incomparable, for the presence of a few exceptionally dull or bright scholars in any group would vitiate the comparison. I therefore planned a second series of experiments, in which four groups were under observation *through a continuous period for two years* (three tests).

The tests used in this second investigation were four in number. The free composition and the word-forming test were retained in their previous form. The mutilated passage test was simplified by the use of two well-known fables, arranged in short sentences. From each fable 15 words were omitted, including verbs, participles, prepositions, adjectives and adverbs, distributed fairly uniformly over the passage, and designed to make marking reliable by counting each correctly-added word as one mark, and each partially-correct addition as half a mark. The fables were printed and each child was given a copy. The omissions were numbered, in order to give the children additional guidance. Thus, the first fable began:

One day a hare met a tortoise (1) was plodding along very (2).
A full mark was given for 'which' in the first omission, half a mark for 'who,' and nothing for such words as 'and,' 'he,' etc.

The fourth test consisted of a list of 12 incomplete analogies, also printed, in which one of the four terms was omitted and had to be supplied. They were presented as follows:

1. Coal and fireplace = tobacco and ...
2. Workman and master = soldier and ...

In marking, it was found necessary to give credit for several solutions which were not anticipated but which were not wholly unsatisfactory. Thus, in

3. Body and arm = tree and ...

the obvious omission is 'branch,' but 'twig' and 'leaf' were accepted, and in

7. Food and body = rain and ...

'river,' 'sea,' 'snow,' 'ground,' and 'plant' were accepted.

No test, with the exception of the free composition, was given without explanation and preliminary practice, and the time granted for each test varied a little with the different groups, and was determined by the speed with which the quicker pupils completed their work. As the comparison is always between two sections of one group this small variation produced no disturbing factor.

A serious difficulty in investigations spread over a long period of time, consists in irregularity of attendance. In one case I found it necessary to defer the last tests for an extra three months because of absentees. The investigator who visits schools at infrequent intervals must accept this evil, but it reduces the total number of subjects considerably.

A second difficulty is the temptation to 'copy,' when group tests are used with 40 or 50 scholars. In three of the four tests used it would have been an easy matter to copy the isolated words and thus score a good mark. To avoid this danger, I arranged the tests in pairs—two mutilated passage tests, two groups of letters for the word-forming tests, and two lots of six analogies in the incomplete analogy test. The test material was then given out so that only alternate children were working at the same exercise; when it was completed their papers were collected and the test material was then distributed in the reverse order. No such precautions were necessary in the free composition, and even where I thought I observed 'copying' no trace of it could be found in the papers when I compared them afterwards. The more backward child, even if he saw and understood a phrase on a neighbouring paper, showed no sign of incorporating it into his own work.

The results were tabulated as follows:

Free Composition Test.

SCHOOL I

Monoglots							Bilinguists						
Subject	First Tests		Second Tests		Third Tests		Subject	First Tests		Second Tests		Third Tests	
	Essay I	Essay II	Essay I	Essay II	Essay I	Essay II		Essay I	Essay II	Essay I	Essay II	Essay I	Essay II
A	—	—	2	3	3	3	A	1	1	2	2	2	2
B	2	3	2	3	2	2	B	—	—	3	3	3	4
C	2	3	3	3	3	3	C	2	2	2	2	2	2
D	1	1	2	2	2	2	D	—	—	4	3	4	3
E	1	1	—	—	—	—	E	—	—	4	4	3	4
F	1	1	—	—	2	2	F	2	2	3	3	3	2
G	2	2	2	2	2	3	G	3	3	—	—	—	—
H	2	2	3	3	3	3	H	3	3	3	4	3	4
I	—	—	3	2	3	3	I	3	3	—	—	3	4
J	4	4	3	4	4	5	J	2	2	2	2	2	2
K	4	4	4	4	—	—	K	2	2	2	3	3	3
L	2	2	3	2	2	3	L	3	3	4	3	3	4
M	2	2	4	3	—	—	M	3	3	4	4	—	—
N	2	2	3	3	3	3	N	3	4	3	3	3	3
O	3	2	—	—	4	4	O	1	1	1	1	—	—
P	3	2	2	3	—	—	P	4	3	4	3	3	4
Q	4	4	4	4	4	5	Q	1	1	—	—	—	—
R	2	2	3	3	3	3	R	4	4	3	4	4	5
S	3	3	4	4	4	4							

It is not necessary to set out in detail all the individual scores, and we may reduce the above results to the following convenient summary:

Monoglots					Bilinguists				
Tests	No. of pupils	Average score	Average increase	Av. increase / Av. score	Tests	No. of pupils	Average score	Average increase	Av. increase / Av. score
1st 2nd	14	5·07 6·07	1·0	0·197	1st 2nd	12	5·0 5·58	0·58	0·116
2nd 3rd	13	5·77 6·15	0·38	0·067	2nd 3rd	13	6·0 6·15	0·15	0·025
1st 3rd	13	4·69 6·15	1·46	0·311	1st 3rd	11	5·27 6·0	0·73	0·138

The final columns in this table, where the average increase is divided by the average initial score, give the best basis of comparison, and the bottom row shows the comparative gain in two years. Thus, in the above example, the monoglots have improved in essay writing more than twice as much as the bilinguists.

The same summary table for the other three schools is given below:

School II

Monoglots					Bilinguists				
Tests	No. of pupils	Average score	Average increase	Av. increase / Av. score	Tests	No. of pupils	Average score	Average increase	Av. increase / Av. score
1st	15	6·0	0·53	0·09	1st	8	6·125	0·125	0·02
2nd		6·53			2nd		6·5		
2nd	26	6·27	0·65	0·104	2nd	13	5·85	0·61	0·104
3rd		6·92			3rd		6·46		
1st	14	5·96	1·0	0·168	1st	7	6·143	0·714	0·116
3rd		6·96			3rd		6·857		

School III

Tests	No. of pupils	Average score	Average increase	Av. increase / Av. score	Tests	No. of pupils	Average score	Average increase	Av. increase / Av. score
1st	13	5·54	1·0	0·18	1st	16	5·625	0·812	0·144
2nd		6·54			2nd		6·437		
2nd	13	6 31	0·69	0·109	2nd	19	6·58	0·68	0·104
3rd		7·0			3rd		7·26		
1st	18	5·5	1·39	0·253	1st	16	5·75	1·31	0·228
3rd		6·89			3rd		7·06		

School IV

Tests	No. of pupils	Average score	Average increase	Av. increase / Av. score	Tests	No. of pupils	Average score	Average increase	Av. increase / Av. score
1st	14	5·36	0·57	0·106	1st	15	6·13	0·47	0·77
2nd		5·93			2nd		6·6		
2nd	16	5·875	0·75	0·128	2nd	19	6·11	0·68	0·11
3rd		6·625			3rd		6·79		
1st	13	5·62	1·46	0·26	1st	15	6·13	1·14	0·186
3rd		7·08			3rd		7·27		

Here, then, we have four groups of scholars, investigated independently and under observation for two years, whose power of free composition develops at a different rate according as they are monoglot or bilingual. The superiority in each case is with the monoglot, and it is significant in amount.

Moreover, the system of marking adopted diminished the vagaries of an artificial standard. This is confirmed by working out the correlation coefficient between the results of each class for successive years. The coefficient is uniformly high, and is usually about ·8, varying but little on either side. A high coefficient would be naturally expected under the conditions described; its constancy seems to indicate that the difference between the two groups is a significant one.

In the word-building tests the results were much less regular. The detailed scores are given, as before, for the first school only, and then summarized for each school.

Word-Building Tests (two attempts).

SCHOOL I

Monoglots				Bilinguists			
Subject	1st year	2nd year	3rd year	Subject	1st year	2nd year	3rd year
A	—	12	9	A	4	8	11
B	4	—	5	B	—	11	13
C	10	13	—	C	3	10	7
D	2	7	6	D	—	9	10
E	7	—	—	E	—	14	24
F	6	8	7	F	6	12	14
G	16	11	16	G	5	12	—
H	11	13	—	H	9	—	10
I	—	9	15	I	9	13	16
J	15	24	[illegible]	J	9	6	6
K	16	19	—	K	10	7	14
L	10	8	4	L	16	—	19
M	15	17	—	M	13	21	—
N	9	14	19	N	18	16	15
O	11	18	12	O	8	7	—
P	8	7	—	P	15	16	—
Q	10	16	16	Q	4	9	—
R	—	4	8	R	15	10	16
S	11	10	5				

SUMMARY. SCHOOL I

Monoglots					Bilinguists				
Tests	No. of pupils	Average score	Average increase	Av. increase / Av. score	Tests	No. of pupils	Average score	Average increase	Av. increase / Av. score
1st	14	10·71	2·5	0·234	1st	13	9·15	2·16	0·236
2nd		13·21			2nd		11·31		
2nd	12	11·75	—0·42	—	2nd	11	10·64	2·73	0·259
3rd		11·83			3rd		13·27		
1st	10	9·4	1·5	0·16	1st	10	9·9	2·9	0·29
3rd		10·9			3rd		12·8		

These results are curiously irregular. Both groups make almost exactly the same relative progress during the first interval, but at the end of the second interval the bilinguists show a marked superiority. There are some surprising retrogressions and spurts in the detailed table, and the correlation coefficient is much lower than in the case of the free composition test. The remaining schools do not reveal any uniform tendency:

SCHOOL II

Monoglots					Bilinguists				
Tests	No. of pupils	Average score	Average increase	Av. increase / Av. score	Tests	No. of pupils	Average score	Average increase	Av. increase / Av. score
1st	14	9·0	0·71	0·079	1st	8	10·75	0·5	0·046
2nd		9·71			2nd		11·25		
2nd	25	8·48	6·4	0·755	2nd	12	8·58	4·84	0·564
3rd		14·88			3rd		13·42		
1st	14	8·79	6·35	0·722	1st	7	11·714	3·143	0·268
3rd		15·14			3rd		14·857		

School III

Monoglots					Bilinguists				
Tests	No. of pupils	Average score	Average increase	Av. increase / Av. score	Tests	No. of pupils	Average score	Average increase	Av. increase / Av. score
1st 2nd	12	7·58 10·5	2·92	0·385	1st 2nd	17	7·3 10·0	2·7	0 37
2nd 3rd	12	9·33 12·25	2·92	0·313	2nd 3rd	16	10·69 16·87	6·18	0·578
1st 3rd	13	6·54 12·0	5·46	0·835	1st 3rd	16	7·94 16·44	8·50	1·07
School IV									
1st 2nd	12	6·67 11·58	4·91	0·736	1st 2nd	13	5·5 10·61	5·11	0·93
2nd 3rd	16	10·25 12·81	2·56	0·25	2nd 3rd	17	10·82 12·35	1·53	0·141
1st 3rd	12	6·67 15·17	8·5	1·274	1st 3rd	12	5·75 12·83	7·08	1·23

It will be seen that in two schools the monoglots, and in two schools the bilinguists have the advantage. The correlation coefficient is low, as a rule, varying from below ·5 to over ·7.

Mutilated Passage Test.

In this and in the analogy test the groups in two of the schools were only subjected to two tests. The results are given below in summary form:

School I

Monoglots					Bilinguists				
Tests	No. of pupils	Average score	Average increase	Av. increase / Av. score	Tests	No. of pupils	Average score	Average increase	Av. increase / Av. score
1st 2nd	12	8·7 13·04	4·34	0·499	1st 2nd	11	9·18 12·86	3·68	0·40
School II									
1st 2nd	15	9·5 13·67	4·17	0·439	1st 2nd	8	9·75 13·56	3·81	0·391
2nd 3rd	24	11·31 16·04	4·73	0·418	2nd 3rd	11	9·86 15·54	5·68	0·576
1st 3rd	12	8·83 15·83	7·0	0·793	1st 3rd	5	12·1 17·5	5·4	0·446
School III									
1st 2nd	14	11·5 16·43	4·93	0·43	1st 2nd	17	9·59 14·56	4·97	0·518
School IV									
1st 2nd	12	8·21 12·25	4·04	0·492	1st 2nd	14	8·89 12·5	3·61	0·406
2nd 3rd	15	11·57 14·27	2·7	0·233	2nd 3rd	17	12·44 15·82	3·38	0·272
1st 3rd	13	7·92 14·38	6·46	0·816	1st 3rd	15	8·97 16·07	7·1	0·792

These results show a superiority of the monoglots over the bilinguists in three out of four schools. The correlation coefficient is uniformly high, and in nearly every case is over ·8.

Analogies Test.

SCHOOL I

Monoglots					Bilinguists				
Tests	No. of pupils	Average score	Average increase	Av. increase / Av. score	Tests	No. of pupils	Average score	Average increase	Av. increase / Av. score
1st	14	4·5	0·286	0·064	1st	12	4·25	0·83	0·195
2nd		4·786			2nd		5·08		
SCHOOL II									
1st	14	3·86	0·35	0·091	1st	8	4·375	1·0	0·229
2nd		4·21			2nd		5·375		
2nd	25	3·24	2·84	0·877	2nd	11	5·364	0·818	0·153
3rd		6·08			3rd		6·182		
1st	13	3·85	2·3	0·597	1st	6	5·833	1·5	0·257
3rd		6·15			3rd		7·333		
SCHOOL III									
1st	15	2·8	2·8	1·0	1st	19	3·37	1·47	0·436
2nd		5·6			2nd		4·84		
SCHOOL IV									
1st	10	3·6	2·6	0·722	1st	13	3·0	1·61	0·537
2nd		6·2			2nd		4·61		
2nd	15	4·73	0·87	0·184	2nd	16	4·375	1·312	0·3
3rd		5·6			3rd		5·687		
1st	10	3·6	3·0	0·833	1st	13	2·857	2·286	0·8
3rd		6·6			3rd		5·143		

These results, again, show a superiority of the monoglots over the bilinguists in three out of four schools. The coefficients of correlation vary between ·5 and ·7.

It is important to add that these results are applicable only to present-day school conditions and organization. The special problem offered in bilingual places, whether in Wales or in the foreign quarters of our large cities, has been too little considered; this is not the place to enter into so difficult an educational question. It is sufficient to suggest here that there is a real problem to be solved by teachers and administrators, and that neglect of it is a serious weakness in our educational methods and results. Bilingualism may yet be shown to be no intellectual disadvantage in the young; but the tests described in this paper clearly support the view that under present methods it is a positive disadvantage.

This view is strikingly supported in two recent articles in *The Journal* *of Experimental Pedagogy*[1], which describe a careful investigation, carried out after that described above, into the intelligence of bilingual and monoglot scholars in Welsh urban and rural areas. Using the Stanford University Binet-Simon scale, as well as the tests which appeared in Binet's scale of 1911 and Burt's English version of Binet's scale which were omitted from the Stanford scale, Saer examined individually 775 children under 12 years of age, and found "a clear superiority" of the monoglot over bilingual children in rural districts. In urban districts he regards the disturbing effects of bilingualism on the growth of intelligence, as tested by the Binet scale, as "not significant," but suggests that there is evidence, even in urban areas, of "mental confusion"; and the differences that he has shown to exist between monoglot and bilingual children he traces entirely to "the use of two languages before a power of using one language effectively has been acquired."

IV.

The following conclusions may be therefore drawn:

1. Monoglot children, between the ages of 8 and 11, make better progress than bilingual children in their power of expression, their choice of vocabulary, and their accuracy of thought. So far from bilingualism being an "intellectual advantage," it seems to be exactly the reverse, at least under the present organization of schools in Wales.

2. This superiority is revealed more consistently by some tests than others, but if the results obtained in four schools are summed and averaged, the monoglot's superiority is established in each of the four tests.

3. As measured by the correlation coefficient, the free composition test and the mutilated passage test are more 'reliable' than the word-building test and the analogy test. The average correlation coefficient in the two first is over ·8; in the two last it is just over ·6.

---

[1] "An Inquiry into the effect of Bilingualism upon the intelligence of young children." By D. J. Saer.

(*Manuscript received* 15 *March*, 1922.)

HAND AND EAR TESTS.

BY HUGH GORDON.

I. *Introduction* (pp. 283, 284).
II. *First series of tests*
    (*a*) *Procedure* (pp. 285–287).
    (*b*) *Class tests* (p. 287).
    (*c*) *Individual tests* (pp. 287–290).
III. *Mental defectives and the tests* (pp. 290, 291).
IV. *New series of tests*
    (*a*) *Procedure* (pp. 291, 292).
    (*b*) *Results* (p. 292).
    (*c*) *Application of tests to epileptics* (pp. 292–294).
V. *The series of tests and intelligence* (pp. 294–296).
VI. *Possible explanation of some of the responses* (pp. 296, 297).
VII. *Theoretical conclusions* (pp. 298–300).

I. INTRODUCTION.

IN his paper on "Aphasia and Kindred Disorders of Speech[1]" Head described a series of experiments on aphasic patients, in which the patients are required to imitate a series of movements when seated opposite the observer. The following quotations will explain the exact procedure.

> "The hand, eye, and ear tests.—First of all the patient, seated opposite the observer, attempts to imitate a series of movements which consist in touching an eye or an ear with one or other hand. Before beginning these tests it is well to make sure that he knows his right hand from his left, and understands the nature of the task he has to perform."
>
> "Some normal persons find difficulty in performing these movements correctly over a series of from sixteen to twenty-four tests; many, however, can carry them out perfectly, especially if they are young and intelligent, and belong to the class from which so many of my war patients were drawn. There is a natural tendency to select the hand opposite to that used by the observer; this error is in most instances checked consciously. But none of the normal men I have examined failed to recognise that when the left hand was in contact with the right ear it had crossed the face;

---

[1] *Brain*, July 1920, XLIII, Pt. 2, 101.

and yet this want of appreciation of a crossed movement was one of the commonest pathological mistakes."

And again:

> "The patient is placed in front of a large mirror, and is asked to imitate the reflected movements of the observer standing behind him. In all normal persons and in most of those suffering from disorders of speech, this can be carried out perfectly; it is an act of direct imitation uncomplicated by considerations of right and left, and not attended by the necessity for internal speech."

In infant schools when a teacher illustrates any movement she wishes the children to imitate, she finds it necessary to use the opposite hand or foot to that required, *e.g.* she must raise her left if she wishes them to raise the right. This fact is well known, and is especially noticeable in drill, and in some of the instruction in drawing and writing.

These facts, taken in conjunction with what Head found, seemed to indicate that a change may take place among older children in the interpretation of the words, "Hold up this hand," made by any one facing them. An experiment was therefore made in a school consisting of two departments—infants (ages about 5–7½ years), and girls (ages about 7½–14 years). In the Infant Department and in Standards I and II (*i.e.* to about 8½ years of age) nearly every child held up the left hand, when asked to do exactly what the observer did, who held up the right hand. In Standards III, IV, V and VI there was some hesitation and an increasing number held up the right hand. Many held up one hand and then changed to the other. In Standard VII nearly every girl in the end held up the right hand. Two girls were asked how they came to do it. One answered, "I put myself in your place"; the other, "I immediately made up my mind to do the opposite." In subsequent class tests children were asked to write down why they held up one hand and not the other. Most of them wrote, "Because it was your right hand"; others, "Because you told us to," clearly indicating that they realised it was the right hand that was raised.

As the response made by the older children differed from that made by the younger, it was evident that a change had taken place between the ages of 8 and 14. More experiments were therefore made in class, and a careful record was kept of the ages and standards of the children tested. It was of course recognised that imitation by some of others in the class might materially affect the percentages of correct responses; this was as a matter of fact found to be the case, when children were tested separately.

II. FIRST SERIES OF TESTS.

(*a*) *Procedure.* The following procedure was adopted in all experiments. The observer said to the class or individual about to be tested: "I am going to give you something very easy to do."

Test 1. "Hold up your right hand."

Test 2. The observer then turned with his back to the class. *After* turning he said: "Do exactly what I do. Hold up this hand," holding up his right hand. After lowering his hand he turned round.

Test 3. Now facing the class the observer repeated: "Do exactly what I do. Hold up...this hand." After saying, "Hold up" he waited for one or two seconds before adding, "this hand," and holding up his right hand. If the child held up his left hand, the observer kept up his hand for two or three seconds, to give the child an opportunity of changing from the left hand to the right.

Test 4. The observer repeated: "Do exactly what I do," moving his left hand across to his right ear. The child was given as much time as he wished to complete his response.

When children were tested separately, they were given the following additional tests, if they had made only one mistake in the four tests.

Test 5. "Do exactly what I do." Both hands were raised, and then the right was lowered. If this was correctly carried out, the following was given.

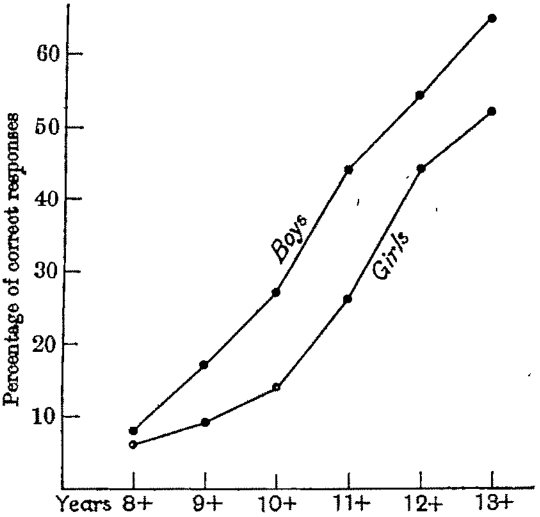
Test 6. Repetition of the test wrongly done, but it was given with the opposite hand.

It will be noticed that these tests differ materially from those of Head, as no intimation was given as to the object of the tests. The experiments were in fact made with the object of determining, how children at various ages would interpret the words, "Do exactly what I do," in tests 2, 3, and 4.

Scoring.—Children were marked as 'correct' if all responses in the four tests were 'correct,' or if five out of six. Nothing more is implied by the term 'correct' than that the same hand is moved as the one moved by the observer, *e.g.* the right hand of the child is raised, when the observer holds up his right hand.

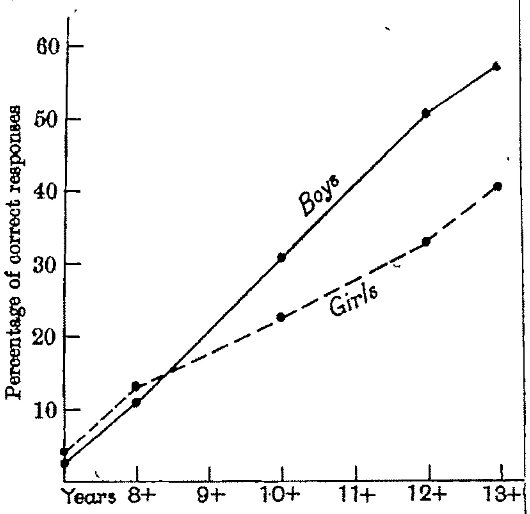
Remarks.—It is important to begin with the words: "I am going to give you something very easy to do," as it tends to remove nervousness in some of the children, and prevents the older ones from thinking that the test is of a kind only suitable for infant schools. Test 1 was merely given to see that they all knew their right hand from their left.

Fig. 1. Class testing.



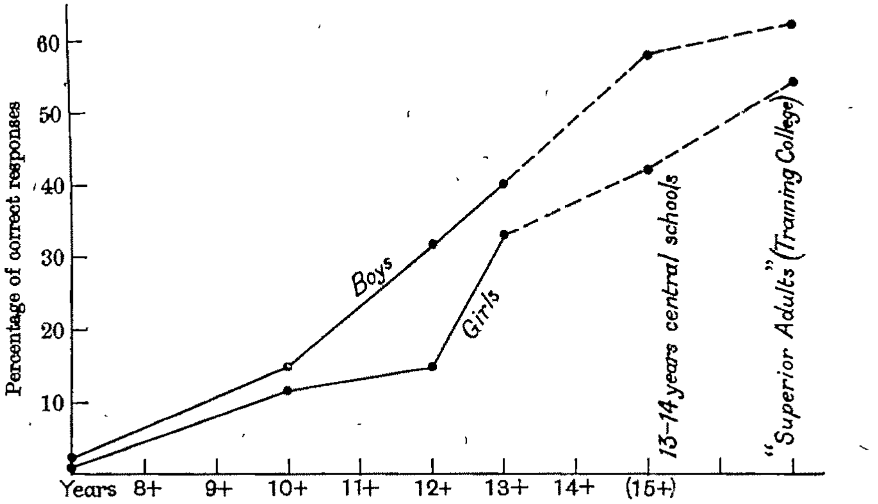
Years	8+	9+	10+	11+	12+	13+
No. of Boys	62	109	158	158	164	171
No. of Girls	102	157	132	147	143	138

Fig. 3. Individual testing, new series.



Years		8+	9+	10+	11+	12+	13+
No. of Boys	60	146		340		284	211
No. of Girls	72	204		243		222	259

Fig. 2. Individual testing, 1st series.



Years		8+	9+	10+	11+	12+	13+	14+	(15+)	
No. of Boys	81			267		307	461		136	29
No. of Girls	87			253		393	373		219	50
No. of Schools	4			7		10	17		5	1

By the time they had done tests 1 and 2, they appeared nearly always to have lost all signs of nervousness—a state that might have interfered with their interpretation of the tests. Test 2 was substituted for Head's experiments in front of a mirror. It was given to see whether the younger children could imitate correctly. As a matter of fact it was found to be a most important part of the test, especially among the older children, and it gave the most unexpected results. Test 3 was the real test of the series; test 4 was added to discover whether test 3 was correctly done, knowingly or merely by chance. Test 5 appeared to be more difficult than the other tests, and it was given so that children, who had made one mistake might have the opportunity of correcting that mistake.

(*b*) *Class testing.* As nearly all young children (*i.e.* under 8 years of age) were found merely to imitate the observer, children from 8 years to 14 were experimented on in the senior schools, to discover at what age the change from holding up the left hand to that of holding up the right had taken place, in response to the words, "Hold up this hand" (*i.e.* the right hand). Fig. 1 gives the result of experiments conducted in many different schools. The ages (*e.g.* 8–9, 9–10, etc.) are given along the base line, and the percentages of correct responses (in all four tests) are indicated by the length of the vertical lines. Underneath are the actual numbers tested. From this table it will be seen that the percentage of correct responses gradually increased with the age of the children, and that in the case of the boys it reached 65 % between 13–14 years of age, and in the case of the girls 52 % between the same ages. These facts seemed to indicate that the series of tests might develop into an 'intelligence test' for 13–14 years 'mental age.' The test fulfils some of the requirements of a good test, as it shows a somewhat rapid but even rise from 9 to 13 years of age in the percentage passing, reaching 50–60 % between 13–14.

These results seemed to justify further investigation. The tests with addition of tests 5 and 6 were therefore made on individual children, who were tested separately, and were not allowed to communicate with each other. There are one or two interesting points in connexion with the curves given in this table. It is clear that at every age the boys had made a greater percentage of correct responses than the girls, even in mixed schools, where the girls were taught in the same classes with the boys. The rapid increases in the percentage of correct responses began in the case of the boys at about 9 years of age, whereas in the case of the girls it began a year later.

(*c*) *Individual testing.* As has been already explained, children were marked as correct who responded 'correctly' to the four original tests (or to five of the six tests). Fig. 2 gives the result of these tests. In many cases mixed schools were purposely tested, and in no instance were schools included unless the tests were completed in a morning or in an afternoon; this was done to prevent any communication between tested and untested children. As there were but few children over 14 years of age in these schools, some experiments were made with children in central schools between the ages of 13 and 14. These children are the picked ones who had obtained scholarships at 10 to 11 years of age, and may be considered to have been on the average in the schools tested about two years in advance of children of the same age in ordinary elementary schools. They have therefore been represented in Fig. 2 as from 15–16 years of age, but the curves have been continued with dotted lines. A few students in Training Colleges have been added as "Superior Adults," and the dotted lines continued. In every school examined all the children of the given age who were present were tested. The curves given in this diagram are the final results, but curves were plotted from time to time; they were found, however, not to vary very much from the final curve. The small increase of correct responses of the girls from 10 to 12 was at first thought to be due to accidental causes, so a larger number was taken at 12 to 13. The slowing down at first seemed to be disappearing, but it reappeared, when a still larger was taken, and when the number at 10 to 11 was increased[1]. In the case of the boys this peculiarity at 10 to 12 never appeared, but their rate of increase was continuous from 10 upwards. Of course this slowing down of the girls might have been due to the removal at 11 of 10 % of the best children by scholarships to Central or Secondary Schools; but, if this was the case, one would have expected to have found the same peculiarity in the boys' curve. The rate of increase of the boys is however the same from 10 to 13 (and 15?), as is seen by the almost straight line. In mixed schools the percentage of correct responses at 12 to 13 years of age by the boys was nearly always about double that by the girls of the same age.

There was one unexpected response made by certain children of ten and upwards, and it was difficult to account for this response. In test 2 when the observer had his back to them, these children held up the left hand, when he held up the right. These same children nearly always did tests 3 and 4 correctly, and without any hesitation. To investigate the matter further they were given test 5 (*i.e.* holding up both hands, and then lowering the right hand); to this they responded correctly.

---

[1] This result was not confirmed by the later series of tests (see Fig. 3).

When the observer again turned his back to them and held up his left hand, they always responded by holding up the right. When asked which hand the observer was holding up, they replied "Your right hand." The observer then turned round, still holding up the same hand, and said, "Which hand?" They still answered "Your right hand." Then lowering his hand the observer requested them to look carefully, and touch his right hand. After a little consideration they then touched his *right* hand. On the observer saying, "You now see your mistake," they replied "Oh yes." The observer again turned his back to them, and repeated, "Now hold up this hand," raising his right hand. With only one or two exceptions they immediately held up the *left* hand. These children appear to be convinced that, when they wish to copy any one, they must do exactly the opposite. Probably this idea is due to imitating movements made in front of them by instructors in drill or to some other cause. These children were not however alert or intelligent enough to deal with the new situation, that is of the observer with his back to them. This seems to be the most likely explanation, as even when they had seen their mistake, they were still unable to use the knowledge they had gained, and in consequence again failed in this test. The percentage of such failures (*i.e.* only in test 2) did not vary very much among children between 10 and 14 years of age (see Table I). No children between 7 and 8 failed in this part of the test.

TABLE I.

		BOYS						GIRLS					
						Centr. schs.						Centr. schs.	
	Age in years	7–8	10–11	12–13	13–14	13–14	T.C.	7–8	10–11	12–13	13–14	13–14	T.C.
	No. tested	81	267	307	461	136	29	87	253	393	373	220	50
Percentage of responses	Correct	2·4	15·3	31·5	40·5	58 8	62	1·1	12·2	15·2	32·9	42·3	54
	Incorrect in 3, 4	86·4	68·9	52·4	43·8	25·7	34·4	81·6	75	70	57·9	44·5	42
	" " 3 only	0	1·8	1·0	2·1	1·4	0	0	0	0	·5	1·3	4
	" " 4 "	1·2	4·1	6·2	5·5	8·0	0	2·3	3·0	2·7	3·0	1·3	0
	" " 2 "	0	5·2	6·5	4·3	5·1	0	0	6·0	5·0	5·6	7·7	0
	" " 1 "	0	0	0	0	0	0	1·1	·7	0	0	0	0
	" " 2, 4	6·0	2·2	3·0	1·9	·7	0	4·6	1·5	4·0	1·3	1·3	0
	" " 1, 3, 4	3·6	·7	1·0	·6	0	0	9·2	·7	·7	·5	·4	0
	" " 2, 3, 4	0	·7	0	0	0	3·4	0	0	·7	·3	·4	0
	" " 1, 2	0	1·5	0	·4	0	0	0	0	0	1·0	·4	0
	" " 1, 3, 4	0	0	0	·4	0	0	0	0	0	·3	0	0
	" " 1, 4	0	0	0	·2	0	0	0	0	·5	0	0	0
	" " 2, 3	0	0	0	0	0	0	0	0	·2	0	0	0
	Test 4												
	R. hand to R. ear	2·4	3·0	3·0	2·1	·7	3·4	2·3	2·2	2·0	1·6	1·3	0
	L. hand to L. ear	4·8	1·5	3·0	3·0	7·3	0	4·6	1·5	4·7	1·0	1	0

The first boy between 7 and 8 years of age to do all the tests correctly was stated to be a mathematical genius[1]; there are very few indeed of this age, who can carry out the tests correctly. In two or three other schools, while children between 10 and 11 were being tested, teachers without any suggestion from the experimenter remarked that the mathematical ones were being picked out. It seems possible that there is some connexion between the ability to pass in the series of tests and a mathematical type of mind. Other facts appear to point to the same conclusion. A great variety of schools was tested, in very poor and in well-to-do districts, but the character of the district did not seem to make much difference. In one school, where the boys are very sharp out of school but poor at school work, the percentage of correct responses was one of the highest.

An analysis of the results of the testing is given in Table I. It clearly shows that nearly all failures were due to failures in tests 3 and 4, *i.e.* the children failing merely 'imitated directly' the movements of the observer. There were very few over 8 years of age, who did not know their right and left hand. One girl had to look at a bangle on her left wrist before she could distinguish her right from her left hand. In test 4 the percentage of children, who did not cross their face with their hand was higher than had been expected (between 5–6 %), and it did not decrease much with age. In responding to the tests, children who failed in tests 3 and 4 reacted quickly, whereas those who did these correctly made no movement for two or three seconds. Those who failed in test 2, generally reacted rapidly and correctly in tests 3 and 4. It was interesting to watch the faces of the children, especially the faces of the younger ones, who reacted correctly, as it was evident in some cases that a struggle was going on to decide which hand the observer was really moving.

III. MENTAL DEFECTIVES AND THE TESTS.

Boys and girls in mental defective schools (up to the age of 16 years) nearly always failed in tests 3 and 4; they merely 'imitated directly.' As a rule children over 8 in these schools did tests 1 and 2 correctly, but there were a great many more than among normal children, who did not cross the face with the hand in test 4. Tested in class these defective children, even the older ones, behaved in a way similar to normal children of 8 or under.

The following results were obtained from a school for mental defectives for older boys. The whole school was tested individually, 48 in the morning, and 53 in the afternoon. There were five under 12 years of age, the rest were between 12 and 16 years old. The average age for the whole school was 13½ years; the number tested was 101, and the number of correct answers was 8 (8 %). Not one of the 22 under 13 years of age passed the test. Of the 79 between 13 and 16 years of age (average age 13 years 10 months) 8 responded correctly, *i.e.* 10·1 %. This percentage is equivalent to that of boys in the ordinary schools of about 9½ years of age (see Fig. 2). The retardation in this test is therefore about 4 years for the older boys in this special school. The following are the reports of the teachers on the 8 boys who passed in this test.

---

[1] His intelligence quotient was found later to be 125 (Stanford Revision Scale).

2 exceedingly good at manual work; one of these the best at wood work in the school;

2 good at arithmetic;

1 sharp at numbers;

1 reading and arithmetic good subjects;

1 reading best subject, but good at arithmetic;

1 arithmetic fair; reading and memory work weak.

IV. NEW SERIES OF TESTS.

A new series of tests was then devised to determine whether with more information children would respond more correctly. The following procedure was therefore adopted. It was clearly indicated in test 1 that when the observer raises his right hand he expects the subject to raise his right hand, for until he does so the question is repeated. When the test is completed correctly the observer says—"very good, that is your *right* hand and this is mine." Test 5 was added as it was at first expected that the four tests would be far too easy, owing to the careful instructions given in test 1. As a matter of fact this additional test was found to be unnecessary, as only a few, who made correct responses to the preceding tests, were unable also to carry out this more complicated one correctly. The results of the tests including this one are given in Table II.

(*a*) *Procedure.* Test 1. Standing on the left of the child—neither in front nor behind—the observer says: "I am going to give you something very easy to do." (*a*) "Which is your right hand?" When this is correctly done, he adds: (*b*) "Which is my right hand?" These responses must be correctly done before passing to the next—(*c*) "When I say, 'Do exactly what I do,' and hold up this hand, which hand will you hold up?" (the observer holding up the right hand). When this is correctly done, but not until then, the observer adds: "Very good, that is your right hand, and this is mine."

Test 2. Turning him at right angles to the first position the observer steps in front with his back to the child and says: "Do exactly what I do," raising the right hand. The observer lowering his hand then turns round[1].

Test 3. Now facing the child the observer repeats: "Do exactly what I do," raising the right hand.

Test 4. Repeats: "Do exactly what I do," moving the left hand across to the right ear.

Test 5. Repeats: "Do exactly what I do," moving the right hand to right ear, and simultaneously touching right elbow with left hand.

Test 6. If *one* test is done incorrectly (*i.e.* in tests 2–5), the observer repeats: "Do exactly etc." raising both hands, then lowering right hand.

Test 7. Repeats, but with other hand, the test incorrectly done.

(*b*) *Results*. The curves for the boys and for the girls obtained by plotting the percentages of correct responses to tests 2–4 at the different ages are given in Fig. 3. The curves are very similar to the previous ones, the girls (with exception of those under 8 years of age) always making a lower percentage of correct responses than the boys. The peculiarity of the girls' curve, between 10 and 12 (see Fig. 2), to which reference has been made, has however disappeared, that is in the latest tests the increase of correct responses, is more or less continuous from 8 years upwards, as it is in that of the boys. It is therefore most probable that this peculiarity was a mere chance due to the small number tested. The instructions given in test 1 have however materially increased the percentage of correct responses for both boys and girls; at 13 to 14 years of age the boys' percentage has risen from 40·5 to 57·2 %, the girls from 32·9 to 40·1 %. The curves cross at about 8½ years of age, that is at this age the boys catch up the girls and then pass them, but the numbers are too small on which to base any definite conclusion. This result however is interesting, as teachers in infant schools and in the lower classes of mixed school have frequently stated that girls about 8 years of age and under appear as a rule to be more intelligent than boys of the same age, but that above this age it is the boys who are the more intelligent.

(*c*) *Application of tests to epileptics*. Dr Tylor Fox, Medical Superintendent of the Epileptic Colony, Lingfield, has tested the epileptic children in the Colony with this series of tests. There were 162 boys and girls between the ages of 5 and 16 years. The numbers at each age were therefore small; the average however did not differ much from that among children in the ordinary elementary schools, and not more than had been found in individual schools. The girls still showed a lower percentage than the boys at every age.

---

[1] After each test (2–7) the observer says "Very good," no matter what response is made.

14 children under 9 years of age gave no correct responses.

19 children between 10–11 years of age gave 21·0 % (normal 27 %) correct responses.

22 children between 12–13 years of age gave 45·4 % (normal 42·0 %) correct responses.

29 children between 13–14 years of age gave 41·3 % (normal 48·5 %) correct responses.

One very noticeable fact, when compared with normal Schools, is the irregularity of the curve with the increase of age, no doubt in part due to the smallness of numbers tested, but probably also influenced by the presence of cases of children that are deteriorating mentally. This irregularity is shown by the following percentages of correct responses of children from 9 years of age to 16; the numbers in brackets are the numbers tested.

9–10 years of age	22·2 %	(9)	13–14 years of age	41·3 %	(29)
10–11 ,, ,,	21·0 %	(19)	14–15 ,, ,,	58·8 %	(34)
11–12 ,, ,,	25·6 %	(12)	15–16 ,, ,,	52·3 %	(21)
12–13 ,, ,,	45·4 %	(22)			

All these children have also been tested by a modified series of Binet tests. Their intelligence quotients by these tests vary from 48 to 130; average for boys 93, for girls 87·2, for both together 91. But Dr Tylor Fox thinks that the results are considerably higher than would be obtained by the standard tests applied in the usual way.

From Table II it will be seen that the great majority of responses are either correct, or that the failures are due to incorrect responses to both tests 3 and 4. Very few indeed fail in test 3 alone; a larger number in test 4 only (1–5 %). It is remarkable, considering the simplicity of test 2, that so high a percentage (2–4 %), fail in it, but respond correctly to the remaining tests. The very high percentage of failures in test 1 (*b*), *i.e.* the inability to point to the observer's right hand, when the child was standing at his side, was quite unexpected. It is as high as 38 % among girls of 8 years of age, and for boys and girls between 13 and 14 years of age the percentage of failures is even more remarkable (9·5 % and 10·8 % respectively).

TABLE II.

Percentages of correct and incorrect responses.

	Boys					Girls				
Age in years	7–8	8–9	10–11	12–13	13–14	7–8	8–9	10–11	12–13	13–14
No. tested	60	146	340	284	211	72	204	243	222	259
Correct in 2–5	1·6	4·0	28·0	41·2	53·0	4·1	7·8	22·6	32·4	38·2
„ „ 2–4	1·6	2·7	2·3	9·1	4·2	0·0	0	0·8	0·9	1·9
Incorrect in 3, 4	91·6	90·4	60·8	41·2	33·6	77·7	87·7	70·7	56·3	52·5
„ „ 3 only	1·6	0	0·6	0·7	0·5	0	0·1	0	0	0·3
„ „ 4 „	1·6	2·0	5·8	5·2	5·2	1·4	1·0	2·4	4·5	1·5
„ „ 2 „	0	1·3	1·7	3·5	2·8	5·5	2·9	2·8	4·0	3·8
„ „ 2, 4	1·6	0	0	0·7	0·5	1·4	0·5	0·4	1·8	0·3
„ „ 2, 3	0	0	0	0·7	0	0	0	0	0	0
„ „ 1 (*b*)	28·3	24·6	14·7	12·6	9·5	8·3	38·7	16·4	12·6	10·8
„ „ 2, 3, 4	0	0	0	0	0	12·5	0	0	0	0·3
Test 4										
R. hand to R. ear	0	1·3	3·5	1·4	1·4	0	1·0	0·8	0·4	0
L. hand to L. ear	3·3	4·8	4·1	2·1	2·8	0	4·4	2·4	6·3	1·1

One very noticeable fact was observed during the actual testing in connexion with the responses to test 1 and to tests 3 and 4. In responding to the question "Which is my right hand?" made by the observer while standing at the side of the child, the subject nearly always, especially in the case of the younger children, turned slightly round and took some little time before answering; it was evident that he was considering the matter. On the other hand, in responding to tests 3 and 4 (*i.e.* when facing the observer) the responses of those who answered *incorrectly* were invariably made rapidly, and almost unconsciously—in fact their movements appeared to be quite automatic, no change in the facial expression of the child being noticeable. Further reference will be made to this important difference.

V. THE SERIES OF TESTS AND INTELLIGENCE.

That correct responses in these tests are as a rule associated with what is commonly understood by intelligence is indicated by the fact that in many schools these tests did pick out the children, who according to the teacher's estimate were the most intelligent, especially those under 11 years of age; in certain schools this was not always the case especially among the older children (12–14 years of age).

In a class in a Junior School all the children had also been tested by the Stanford revision tests for 'intelligence,' and the following are the results obtained.

Number tested: 18 boys, 20 girls. Ages 6 years 2 months, to 8 years 7 months. Intelligence quotients 81 to 125; average I.Q. 104.

The two children heading the list with I.Q.'s 125 (boy) and 124 (girl) together with one other girl—14th on the list, I.Q. 107—were the only ones, who responded correctly to these 'hand and ear' tests. It may be added that the teachers had criticized the position of this last girl[1] on the list of I.Q.'s, as they considered her one of the brightest in the class, and certainly not the 14th. In another School there were 29 girls (13–14 years of age) in Standard VII; 19 responded correctly, and 10 incorrectly. The headmistress was much struck by the result, as the 10 had been in a backward class during the previous year, but had been moved into Standard VII, owing to re-organization of the school. Of course, the above are only given as examples, but Table III points to the same conclusion; it gives the percentage of correct responses of girls and of boys between the ages of 13 and 14 years, of 12 and 13, and of 10 and 11 found in Standards VII to II.

An average scholar should reach Standard VII at about 13 years of age, and Standard VI about a year earlier, and so on to Standard II reached between 8 and 9. One would therefore expect to find, very approximately it must be admitted, children of 13–14 years' 'mental age' in Standard VII, and children of a mental age of 8–9 in Standard II. Such a result however is never attained, owing to many difficulties with which the teachers have to contend. The actual numbers tested are small, in some instances very small, *e.g.* in Standard VII there were only 3 girls of 10 years of age; of course very few of this age are found in Standard VII. Not a single one of the children of 12–14 years in Standard III responded correctly to the tests; others of the same age in this standard were tested in various schools but were not entered in this table, as all the children of this age had not been tested.

The percentages given in this table, even with such small numbers are on the whole consistent, showing an increased percentage with a higher mental age. The exceptions appear to be due to the few tested, *e.g.* girls 13–14 have a higher percentage for Standard IV (22·2 %) than for Standard V (12·5 %), etc.

Some experiments were made in a Training College for men; 57 were tested with this new series, giving 71·9 % of correct responses. After an interval of a quarter of an hour 57 more were tested, the percentage now rose to 89·4 %. Whether the interval had anything to do with this increased percentage is not known; no request had been made as to communications with each other. The percentage for the 114 was 80·7 %.

---

[1] The I.Q. of only one other was adversely criticised as being too high on the list, the results were otherwise thought to be very just.

In contrast to this the results obtained from 125 children in two special schools (for mental defectives) are given. There were 60 boys and 65 girls mostly between 8 and 12 years of age (7 between 12 and 16, and 2 between 7 and 8 years of age). Only one (·8 %), a boy between 9 and 10 years old, responded correctly; 20·8 % did not know their right hand; 15·2 % did not cross the face with their hand in test 4, and 1·6 % failed in test 2 only. These results are remarkable, when compared with those of normal children, who at 7–8 years of age give about 3–4 % correct responses.

TABLE III.

Percentage of 'correct' responses in the different standards for the following ages (*a*) 13–14 *years,* (*b*) 12–13 *years,* (*c*) 10–11 *years.*

Note. *The small figures in brackets are the actual numbers tested.*

			BOYS			
	Standards					
Age	II	III	IV	V	VI	VII
13–14 years	None	0 (3)	0 (7)	43·3 (30)	44·0 (59)	76·4 (68)
12–13 ,,	None	None	30·0 (20)	22·8 (35)	50·8 (61)	41·0 (41)
10–11 ,,	16·6 (6)	22·2 (28)	18·4 (76)	21·4 (42)	55·0 (90)	85·7 (7)
			GIRLS			
13–14 years	None	0 (7)	22·2 (18)	12·5 (24)	38 (34)	58·8 (85)
12–13 ,,	None	0 (13)	14·2 (35)	30·7 (62)	50·0 (64)	60·0 (25)
10–11 ,,	0 (6)	10·8 (92)	14 6 (82)	47·7 (44)	68 9 (16)	66·0 (3)

These facts seem to indicate that the test does give an estimate of the stage of mental development, which a child has reached in some—perhaps restricted—area. Used as one of the many 'intelligence tests' it should be placed among 14th year (mental age) for boys, and among those for 'adults' in the case of girls.

VI. POSSIBLE EXPLANATION OF SOME OF THE RESPONSES.

Test 1 (*b*). The remarkably high percentage of incorrect answers made by children, who pointed to the observer's left hand, when asked to say which was his right hand, observer and child standing side by side, appeared in most cases to be due to placing themselves 'in thought' in front of the observer face to face, and then choosing the hand directly opposite their right hand. In fact many children actually moved in this way and had to be brought back to their original position. A fairly high percentage, especially among younger children found the greatest difficulty in answering this question, and spent some time in making up their minds. In a few cases, it was impossible to get a correct answer. One boy could never consistently answer this question, or hold up the correct hand for the last part of test 1. The headmaster then said: "This explains why we have had such difficulty in teaching him to drill, he never seems to know right and left."

Test 2. The explanation of incorrect responses in this test has already been suggested (p. 289); further it would seem probable that the consistently opposite movements had become quite automatic through an acquired neural pattern. In this new series of tests it is not improbable that the correction of the child's response to test 1 (*b*) may have led in some cases to confusion in his mind, and induced him to respond in an opposite manner to the tests that followed—that is in a way opposite to what he would have done, if left uncorrected.

Test 3. The few, who responded incorrectly to this test but correctly to test 4, probably did so by mere chance; they merely used the same hand for both tests.

Test 4. The reverse of the last (*i.e.* incorrect for test 4 but correct for test 3). The reason for these responses was quite clear in some cases —the right hand was used in all movements. This was proved by the fact that even when the left hand was used in any movement, the children moved the right. The non-crossing of the face was generally found among the very unintelligent; it was certainly far more prevalent among mental defectives. Why this is the case it is difficult to say, unless it is really because of its increased complication.

Test 5. A few who responded correctly to all the other tests persisted in crossing the face with the right hand in this test. It is just possible that the influence of test 4 may have brought about this curious result.

Test 6. This test—raising both hands and lowering the right, seems by far the most difficult, even for children who clearly understand what is expected—they lowered the left hand automatically, and then corrected themselves. The correction often took place when the hand was half-way down. This movement was clearly due to the tendency to direct imitation, which was too strong for the moment.

VII. THEORETICAL CONCLUSIONS.

The contrast, especially among younger children, between the responses in test 1 (*b*) and tests 3 and 4 seems to point in tests 3 and 4 to a very strong tendency to 'direct' imitation—an inherited automatic action, in which the 'higher centres' are not involved. These tests, it will be recalled, are:

Test 1 (*b*). "Which is my right hand?" A question asked by the observer, while standing on the left hand side of the child; and

Tests 3 and 4. "Do exactly what I do," the observer standing facing the child.

In the first test, it is not a matter of imitation but of deciding which is the right hand of the observer. As is seen by the child's facial expression and by the time taken to answer, it is clearly a question that has excited thought and necessitated a decision—a decision that is very often incorrect. In tests 3 and 4 on the other hand, when *incorrectly* done, it is equally evident from the rapidity of the response, and from the lack of any alteration in facial expression, that the movements are automatic. The stimulus has merely excited a lower, primitive, inherited psycho-physical mechanism not involving the 'higher centres.' With the older children it is clear that a higher set of nerve centres becomes involved, centres which gradually assume ascendancy over the lower. The primitive direct reaction is more and more inhibited and supplanted by an entirely different one. The inherited neural pattern, however, is sometimes so strong, even in cases where the impulse has spread to the higher centres, that the direct imitation movement is actually made, and has to be inhibited before a fresh start is made.

There are probably cases among the older children, where they do understand the real point of the test, but are unable to decide correctly, which hand the observer has raised. Such children will take time in responding and the response may be correct or incorrect according to circumstance.

If these conclusions are justified, it is possible that we may be able to estimate to some extent the stage of development a child has reached in this particular respect, *i.e.* when the higher nerve centres are assuming ascendancy over the lower. It also seems probable that this development is closely associated with intelligence. Further, since the boys do better than the girls at every age it follows that in the case of the boys the higher nerve centres become involved and assume ascendancy at an earlier age than in the case of the girls. If this assumption is correct and if intelligence is associated with such development, one would naturally expect the boys to be more successful than the girls in the Binet and other 'intelligence' tests. But this does not appear to be the case, as Burt, Terman and others find that on the average the girls test 'higher' than the boys; the difference, however, is slight. An explanation of this better result in the case of the girls may perhaps be found, when a survey is made of the actual tests in which the girls are the more successful and of those in which the boys are more successful. The following table extracted from Burt's results (*Mental and Scholastic Tests*, p. 194) shows in detail the tests in which the girls do better than the boys, and *vice versâ*. The tests at the top of the lists are those in which the greatest differences are found.

TABLE IV.

Tests that are relatively easier for (a) girls, for (b) boys.

(*a*) GIRLS MORE SUCCESSFUL

Repeating 16 syllables
Naming 4 colours
Reading (and facts)
Dictation
Transcription
Repeating 26 syllables
Definitions (class)
Repeating 10 syllables
,, 6 numbers
,, 3 ,,
,, 5 ,,
Others in which the difference is not so marked, *e.g.*:
3 Rhymes
Sentence Building
Mixed Sentences, etc.

(*b*) BOYS MORE SUCCESSFUL

"Suggestion"
Naming 60 words in 3 minutes
Drawing a square
Carrying out a triple order
Arranging 5 weights
Pence and halfpence
Right and left
Arranging 2 weights
Divided card
Memory drawing
Naming of coins
Folded paper test
Counting from 20 to 1
Drawing diamond
Naming 4 coins
Problems
Others in which the difference is not so marked, *e.g.*:
Absurdities
Differences (concrete) etc.

Burt (*ibid.* pp. 193–196) considers that the sex differences in general ability are much smaller than they are usually supposed to be, but that the girls are much better at reading, spelling, handwriting, and composition, whereas the boys are better at arithmetic, and drawing; and again that the girls have better memories than the boys, and are better in verbal, and linguistic tests, and that the boys are better in practical tests, at money tests, and tests demanding observation and shrewdness. He also says (*ibid.* p. 302):

> "Among the children of ordinary elementary schools girls are decidedly inferior to boys in subtraction and division. In addition, and still more in multiplication,

> they are often superior. This disparity originates chiefly in the dependence of these latter tasks upon rote memory for tables, rote memory being a capacity in which boys always yield to girls."

To what do these facts point? They seem to indicate that automatic reactions are developed earlier in the case of girls than in that of boys, but that the development, in which the higher nerve centres become involved and assume ascendancy, comes at a later age. With this earlier development of automatic reactions may be associated the earlier physical development of girls.

(*Manuscript received* 15 *June*, 1922.)

SOME OBSERVATIONS ON CONTRAST EFFECTS IN GRADED DISCS[1]

By ROBERT H. THOULESS.

(*From the Psychological Laboratory, University of Manchester.*)

If a white four-rayed star be mounted on a black background and rotated on a colour wheel, a curious contrast effect is observed. The disc so obtained is white at the centre and black at the outside with a smooth (but not uniform) gradation of grey between. The contrast effect consists of (i) a white ring more intense than the white centre immediately inside the grey area and (ii) a more intense black ring immediately outside the grey. These rings are also found in a photograph of the rotating star. Fig. A is a diagram of a star of the form described which shows clearly the two rings when it is rotated. One is struck by the difference between these rings and the contrast effects obtained when a white and a black surface are adjacent to one another. The rings appear much more convincingly objective than the ordinary black-white contrast effect. A striking testimony to the objectiveness of their appearance is afforded by the fact that the photographers of my discs were satisfied that the rings were physically present on the plates, and they resented my suggestion that they were merely optical illusions as implying a reflexion on the truthfulness of their cameras. It is, however, easy to prove that (as we might expect) the rings on the photograph have no physical existence. For a simple method of showing this I am indebted to a suggestion made by Professor Bragg. If the photograph be cut through the middle and the two pieces be placed side by side but slightly displaced laterally, then the white ring of one half will be adjacent to the plain white surface of the other. If now the grey part of the disc be covered over, the white of the ring will be seen to match exactly the central white adjacent to it. The subjectivity of the black ring can be demonstrated in the same manner.

The conditions which produce the present effect are not, as in ordinary simultaneous-brightness contrast effects, adjacent surfaces of different brightness values. Over the surface of the disc there is either uniformity of brightness or continuous gradation, never an abrupt change in brightness. There is, however, on this disc, at the beginning and end of the grey, a condition sufficiently analogous to the abrupt change of brightness in ordinary contrast to lead us to suspect it as the cause of the phenomenon. This condition is an abrupt change in the rate of change of brightness (in other words, an abrupt change in the brightness gradation).

---

[1] Read at a Meeting of the British Psychological Society in Manchester, 15 July, 1922.

Perhaps the distinction between a *change of brightness* and a *change in the rate of change of brightness* can be made most clear by a use of mathematical symbols. Let s be the brightness of the sensation received from a point on the disc at a distance r from the centre. Then ds/dr will be a measure of the brightness gradation at that point. The condition for ordinary contrast is that s abruptly changes its value at some value of r. I will try to show that in the smoothly graded disc, the contrast rings result from an exactly similar discontinuity in the function ds/dr. Over the white centre of the disc, ds/dr is zero, for s does not change its value with increasing values of r. Where the star begins, however, ds/dr has a finite value (negative because the brightness decreases with increasing distance from the centre). Similarly, there is an abrupt change from a negative to a zero value of ds/dr at the part of the disc corresponding to the points of the star. Thus although there is a smooth gradation through different values of s as we pass from the centre to the circumference of the disc, there are two positions of discontinuous change in ds/dr. It is at these two positions that the white and the black ring occur.

In order to test my hypothesis that this discontinuity produces the rings, I have prepared variously graded discs by modifying the shape of the star. In these I shall show that (provided that the change in ds/dr is sufficiently large) rings can be obtained in other parts of the disc by producing a similar discontinuity in different parts of the star, and that the rings can be made to disappear if, in discs otherwise resembling the one shown in Fig. A, the passage from a finite to a zero value of ds/dr is made continuous either at the cusps of the star or at its apices. Before describing these, however, it will be worth while to calculate the value of ds/dr in different parts of any star of given dimensions since I shall have occasion to use later the formula so obtained.

In the diagram on p. 304, A is a point of the star, B is a cusp, C is a point at a distance r from the centre O whose stimulus brightness i, when the disc is rotating, we require to calculate. This brightness may

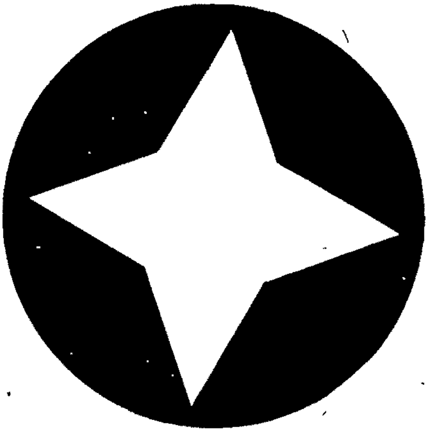


Fig. A

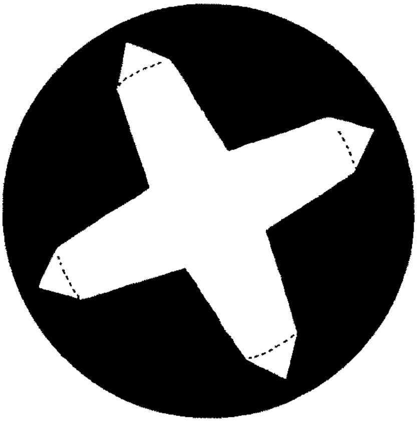


Fig. B

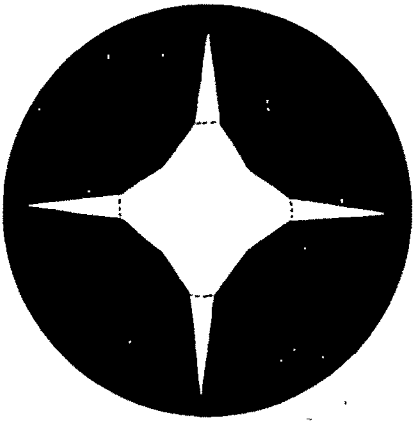


Fig. C

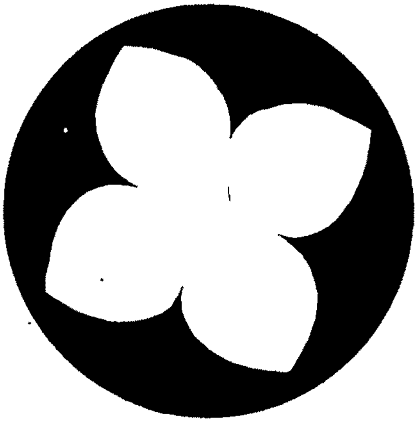


Fig. D

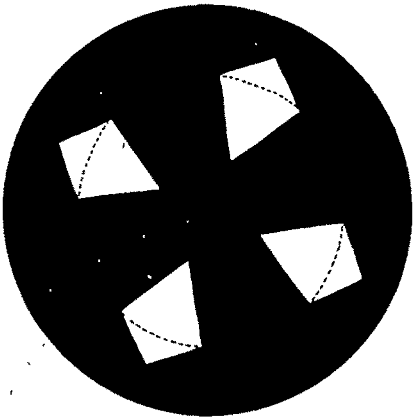


Fig. E

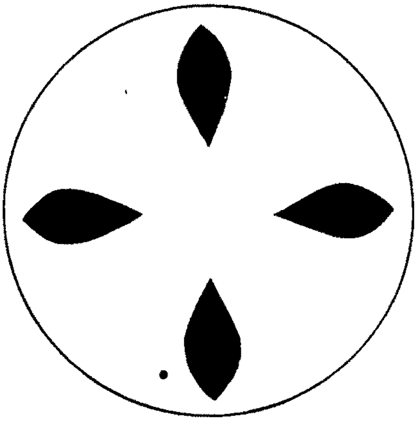
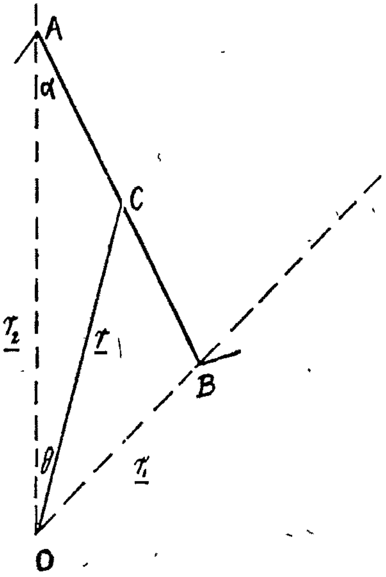


Fig. F

The dotted lines on Figs. B and E show the position of the white rings, those on Fig. C the position of the black ring.

be expressed as the proportion of baryta paper to dead-black required on a colour wheel to match the grey of the disc[1]. My discs have been made by mounting baryta paper on a nearly black background which gave on the colour wheel 27° *W* 333° *B* (*i.e.* for the background $i = \cdot 075$). As might be expected, on the blackest paper procurable the dark ring



does not show clearly; on a true dead-black it could not be expected to be visible at all. Other values required for the calculation are

γ = stimulus brightness of background,
r_1 = distance of cusps from centre (or OB),
$\alpha = \frac{1}{2}$ angle of point of star,
and $\theta = \angle AOC$

(r and θ are not of course independent variables but the formulae are simpler if both are kept in and they are dealt with by partial differentiation).

---

[1] It is necessary to remember that the numerical values in the following calculation have no absolute significance since they do not take into account the effect of varying illumination both on s and on ds/dr. They are only of value for comparing values of ds/dr on different discs under the same conditions of illumination.

The total length of standard white in the grey at C is

$$8\{r\theta + \gamma r(\pi/4 - \theta)\} = 8r\{(1-\gamma)\theta + \pi/4\,\gamma\}$$

$$\therefore\ i = \frac{8r\{(1-\gamma)\theta + \pi/4\,\gamma\}}{2\pi r} = \frac{4(1-\gamma)\theta}{\pi} + \gamma \quad \ldots\ldots\ldots(1)$$

$$\frac{di}{dr} = \frac{4(1-\gamma)}{\pi}\cdot\frac{d\theta}{dr}.$$

To obtain $\frac{d\theta}{dr}$ from the $\triangle\ AOC$

$$\frac{r}{\sin\alpha} = \frac{OA}{\sin\angle ACO} = \frac{OA}{\sin(\alpha+\theta)}$$

$$\therefore\ r\sin(\alpha+\theta) = OA\,.\,\sin\alpha$$

by partial differentiation

$$\frac{d\theta}{dr} = -\frac{\sin(\alpha+\theta)}{r\cos(\alpha+\theta)} = -\frac{\tan(\alpha+\theta)}{r}$$

$$\therefore\ \frac{di}{dr} = -\frac{4(1-\gamma)\tan(\alpha+\theta)}{\pi r} \quad \ldots\ldots\ldots\ldots\ldots\ldots(2)$$

This gives the stimulus gradation at C.

In order to obtain from this the sensation gradient, it is necessary to use Fechner's formula $s = K\log i$.

Then $$ds/di = K/i \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(3)$$

and $ds/dr = di/dr\,.\,ds/di$ which (from 2 and 3)

$$= -K\frac{4(1-\gamma)\tan(\alpha+\theta)}{\pi r i}$$

and (from 1) $$= -K\frac{4(1-\gamma)\tan(\alpha+\theta)}{4r(1-\gamma)\theta + \pi r\gamma}.$$

The constant K can be eliminated by taking as our unit of sensation, the sensation received under the conditions of the experiment from pure baryta paper.

Then ds/dr $$= -\frac{4(1-\gamma)\tan(\alpha+\theta)}{4r(1-\gamma)\theta + \pi r\gamma}.$$

The two values of this function which are of importance for the present purpose are those at the cusps and at the apices of the star. These are

At the cusps $ds/dr = -\dfrac{4(1-\gamma)\tan(\alpha+\pi/4)}{\pi r_1}$.

At the points $ds/dr = -\dfrac{4(1-\gamma)\tan\alpha}{\pi r_2\gamma}$.*

---

* An odd conclusion which would appear to follow from this formula is that if γ were zero (*i.e.* if the background were dead-black) ds/dr at the points would be infinite, whatever value α might have. This seems improbable and one expects a fallacy as, in fact, there is. The fallacy lies in the fact that Fechner's law has been assumed to be true for all values of the stimulus, whereas it ceases to have any significance for less than threshold values

Which for the star *A*, in which $r_1 = 2{\cdot}65$ and $r_2 = 7{\cdot}3$, reduce to $-{\cdot}83$ and $-{\cdot}72$ respectively

In the result produced by the rotation of the star *A*, it will be noticed that the sudden change in stimulus gradation which appears to be the determining cause of the white ring is from zero to a negative value, that which seems to produce the black ring is from a negative value to zero. Sudden increases and decreases in the rate of gradation I shall call positive and negative gradation discontinuities respectively. The evidence of this first disc leads us to suspect that a positive gradation discontinuity produces a black ring while a negative gradation discontinuity produces a white one. To test this hypothesis, I made two stars, Figs. *B* and *C*, in which the arms changed suddenly their angles of convergence so that on rotation there should be respectively a negative and positive gradation discontinuity in the grey part of the disc. In accordance with expectation a white ring is to be seen where the discontinuity occurs in Fig. *B*, and a black one where it occurs in Fig. *C*.

It is also possible to produce a smoothly graded disc in which either the white or the black ring is eliminated by replacing the angle by two exactly touching curves. The disc in which the white ring is thus eliminated is reproduced as Fig. *D*.

It is not difficult to devise many other varieties of graded discs showing these effects. I illustrate only two more. Fig. *E* was made by placing on a black background four symmetrical rectilinear figures cut from white paper in the positions occupied in Fig. *A* by the arms of the star. In this way a disc is obtained in which there is a change in the grey part of the disc from a positive to a negative value of ds/dr. This is a negative discontinuity and a white ring is observed. A black ring can be obtained by placing similar black shapes on a white ground. Fig. *F* is a similar disc obtained by making the black papers of the shape of two intersecting curves. The result of this is that two white rings are obtained but no black one, for there are two negative gradation discontinuities but no positive one.

The result here obtained from the disc *F* might be urged against W. McDougall's drainage hypothesis. This he states as follows: "I regard the phenomenon [of the smoothly-graded disc which I (R.H.T.) have labelled Fig. *A*] as evidence that the more intensely stimulated nerve tract inhibits the less intensely stimulated by drawing from it some part of the living energy set free in it[1]." If it were conceived that McDougall

of the stimulus. The formula worked out will only hold up to the points of the star if the difference between the brightness of the background and a dead-black is above the sensory threshold.

---

[1] *J. of Physiol.* 1903, XXIX. p. xix

meant by this that the presence of the white ring was dependent on that of the black ring and *vice versa*, then the result just obtained would be evidence against his theory. There seems, however, to be no sufficient reason for supposing that he does mean this. The drainage hypothesis would be sufficiently met if the grey part appeared slightly darker than would be indicated by its composition in degrees of white and black. Whether or not this is the case we have no means of testing. In fact, so far as I can see, no experiment on these phenomena can be devised or even imagined which would have any bearing on the drainage hypothesis. If we admit that the meaning of a hypothesis depends on some fact which would be altered by the truth or falsity of the hypothesis, we must conclude that for these phenomena the drainage hypothesis has no meaning.

Lastly, too small a step at the gradation discontinuity results in the disappearance of the ring. This is illustrated in the attempt to obtain these effects by rotating a black ring on a white ground. I found that the black ring inside the grey was visible but there was no white ring outside. This seemed explained by the fact that, as was apparent to observation, the gradation step at the junction of the white with the grey was very small. In fact, for some distance inside the points of the star, the grey was not appreciably different in brightness from the white margin. This smallness of the gradation step was confirmed by calculation which showed that for this star ds/dr at the inside margin of the grey was 1·12, while for the outside margin it was only ·055.

I have (at the suggestion of Dr Myers) made some experiments with discs prepared by rotating coloured stars on a background of the same intensity of grey. I hoped to obtain rings showing a colour contrast due to discontinuity in the colour gradation. Such rings, however, were not formed.

I have also tried to obtain similar colour contrast effects by making discs with a green and with a blue star on a red ground. No effects in colour contrast were obtained although brightness contrast rings were seen when the colours differed in brightness.

A further line of research suggested by the present work would be to discover whether similar gradation discontinuities give contrast effects in the successive presentation of brightnesses or in sounds. The 'cushion of silence' following the cessation of a continuous sound corresponds to the ordinary brightness contrast due to adjacent surfaces of different brightnesses. Can similar cushions of sound or silence be obtained by negative and positive gradation discontinuities in a continuously varying sound?

THE INTERPRETATION AND PLOTTING OF OUTPUT CURVES[1]

BY ERIC FARMER.

IN this paper I propose to deal only with output curves representing performance at various periods of the working day. I have excluded daily, weekly, and monthly output curves, and also curves representing differences between day and night work, or between spells of different length. I have done this partly for the sake of simplicity and partly because each type of curve presents a different set of problems. The problems connected with the more lengthy periods are so complicated and introduce so many other factors than that of fatigue that it would be impossible to deal with them in the short time at my disposal, though any satisfactory treatment of the relation between fatigue and output curves would of course be inadequate without a consideration of curves representing varying periods.

1. THE INTERPRETATION OF OUTPUT CURVES.

Attempts have been made by certain laboratory experimenters accurately to measure fatigue by obtaining a coefficient of fatigue. The formulae employed differ[2] according as different units of efficiency are used, and also according to the nature of the tests from which the data are obtained, but the general principles involved are the same. A comparison is made between the performance in the early part of the test and the performance during the latter period, and by this method a coefficient of fatigue, claiming to measure the actual amount of the subject's fatigue is calculated. I do not propose to criticize these methods in themselves but merely to offer certain reasons why they are not so directly applicable in industrial psychology.

Curves of output are regarded by some as directly comparable to fatigue curves obtained under laboratory conditions, and sometimes such curves are treated simply as fatigue curves. There is a certain amount of justification for this, inasmuch as laboratory fatigue curves and output curves are both curves of performance, but the factors which differentiate them are more important than those which they have in common. A fatigue curve proper is obtained by getting subjects to perform a test calculated to produce fatigue fairly rapidly. Such a test is not the ordinary occupation of the subjects, but is the invention of a psychologist who has very different ends in view from those who invent industrial processes. Such tests cannot possibly have the same meaning for the subjects who perform them as an industrial process has for operatives who have been performing it many hours a day for many years. Attempts are, of course, made to measure the effect of improvement in any performance test, but however accurate such measurements are, it cannot be expected that a laboratory subject will be in the same state of stability as the factory worker. In the one case effort is probably the chief factor making for a high standard of performance, and in the other it is probable that habit plays a far greater part in producing satisfactory results than effort does. The variations in output curves, although in some cases quite as marked as those usually found in fatigue curves, are as a general rule more irregular and therefore more difficult to interpret; and it is the object of this paper to offer a few tentative suggestions as to the best method of interpreting such variations.

---

[1] Read before Section J (Psychology) at the Hull Meeting of the British Association, September, 7, 1922.

[2] *Mental Fatigue* by Jsuru Arai New York: Columbia University, 1912, 81.

The general principle I wish to suggest is, that at least in our present state of knowledge, we should make no attempt to treat output curves as pure fatigue curves and obtain from them coefficients of fatigue. We must endeavour to interpret them in conjunction with other factors and treat them rather as signs of fatigue than as direct measures of it. The attitude of the psychologist who is studying industrial fatigue should be similar to that of the doctor who is endeavouring to diagnose a disease and prescribe a remedy. The doctor does not endeavour to measure mathematically how ill a patient is, but to decide from various symptoms what illness he is suffering from, and to prescribe a remedy calculated to cure the disease. The choice of such a remedy is something in the nature of intelligent empiricism, the value of which will be determined by a change in the symptoms after it has been applied. I suggest that the psychologist is in the same position when examining output curves. His skill will be shown not in measuring fatigue, but in determining its causes and suggesting alterations in the method of work calculated to lessen it.

If an alteration is made in an industrial process and is unaccompanied by any other innovation calculated to increase output, and if that alteration actually does result in an increase of output, we may judge the fatigue value of the alteration, to some extent, by the change in the shape of the output curves. Any one of the following changes in the shape of the output curve may take place.

(1) The output curve may remain the same shape but be on a higher level. When this is the case, it seems to indicate that the new method of doing the work is quicker (and possibly easier), so that the time per unit of output can be reduced, whereas the increased number of operations which this makes possible has the same fatiguing effect upon the worker as the lesser number of more difficult operations. In other words, a greater output has been obtained with the same amount of effort. The fatigue effect of the day is the same, though presumably the effort required for each unit of output is less.

(2) The output curve may be practically on the same level but of a far better shape. When this takes place we may assume that the operation has not been made easier to perform in the sense that it can be done more quickly, but that the cumulative effect of its constant repetition is less fatiguing than that of the original method.

(3) The output curve may be on a higher level but of a worse shape. If this takes place we may assume that the increase in output is due to a quicker method of working, but that this new method is more fatiguing to the worker than the original method. We should expect a result of this kind to follow where methods of 'speeding up' are introduced which pay little regard to the health of the worker.

(4) The output curve may be on a higher level and also of a better shape. If this takes place, we may assume that the increase in output is due to an easier and quicker method of working and that the new method is so much less fatiguing to the worker, that he suffers less fatigue during the day although he is repeating the operation an increased number of times.

We have now to consider what is to be our criterion for judging whether the shape of a curve is good. As has already been pointed out, we are not in a position at present to give any mathematical formula for arriving at a coefficient of fatigue, so that we must content ourselves with some general considerations. If the irregularities in an output curve are too great, we may suspect that fatigue is present in a high degree. Such variations are often a sign that the work is being done mainly by voluntary effort and that the factor of rhythm is not brought sufficiently into play. Some workers have a tendency to work in this way, making their work depend mainly upon quick spurts. Whenever they can be prevailed upon to adopt a more even way of working, the results are invariably satisfactory, though at times they find difficulty in acquiring the new motor habits necessary to effect the change. It must not be supposed from this that the best output curve is one entirely devoid of variations. In practice such a curve is never found and it seems unlikely that even from a theoretical standpoint it is desirable. Rest is probably obtained by a change of rhythm during the working spell; but such changes, if they are having a beneficial effect, will not shew themselves in marked irregularities but only in a normal variation around a mean.

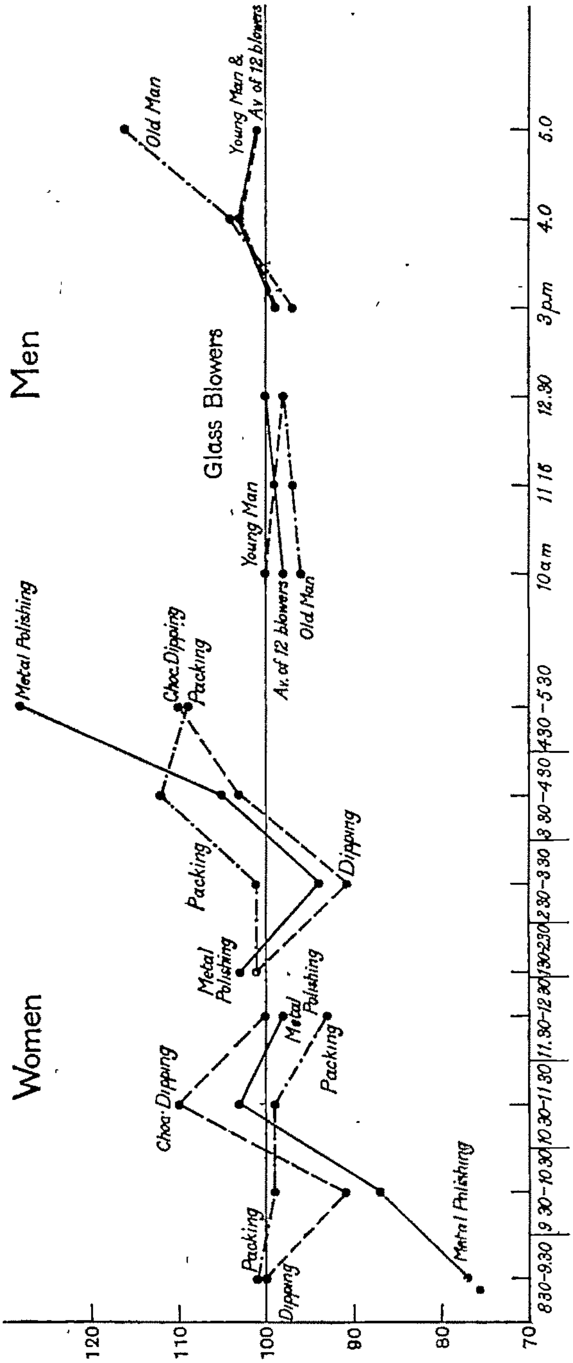
If the drop throughout the day which manifests itself in most output curves continues without any check, it is a bad sign. In any healthy curve of output there are checks in the fall from time to time and sometimes the highest peak is reached two or three times during the day. It is doubtful whether we ought to consider the absence of an end spurt as a sign of considerable fatigue. It is not often that one comes across a curve with a well-marked and genuine end spurt; and if we are to regard this as an indication of excessive fatigue, practically all industrial processes will have to be condemned as too fatiguing. Although we cannot regard the absence of an end spurt as a bad sign, I think that we are entitled to regard the presence of one as a good sign, inasmuch as it shows that the worker still has a considerable amount of energy at his disposal.

2. THE PLOTTING OF OUTPUT CURVES.

The custom of plotting output curves on various scales determined by the nature of the particular operation in question may lead to error in their interpretation. The object of graphic representation is to appeal to visual imagery; and most of us depend to such an extent upon our visual imagery that although we may, when first examining any particular graph, pay attention to its purely mathematical aspect, yet there is a danger that our judgment may be unconsciously biassed by its effect upon our visual imagery. In this way a peak on a graph plotted on a small scale may strike our imagination more, and seem more worthy of consideration, than a smaller peak on a graph plotted on a larger scale. Naturally no rules can be laid down as to the scale on which an output curve should be plotted. The purposes which such curves are made to serve are so various that the particular method adopted will be determined by the particular end in view. When, however, such curves are used with a view of throwing light on the problem of fatigue, it seems not unreasonable to suggest that they should be plotted on a uniform scale. If this is done, the errors that may creep in through the unconscious influence of our visual imagery can be avoided; and—which is even more important—each curve becomes directly comparable with any other curve. The method I have adopted is to reduce each output curve to a curve representing the unit of time required to perform each particular operation. The average of these units of time has been taken as equal to a hundred; and then the times required to perform the operation at various periods of the day have been worked out as percentages of this average. All percentages less than a hundred will therefore indicate that the time required to perform the operation is less than average, and all percentages above a hundred will indicate that it is more than average. By this method we can plot all output curves on the same scale and get a direct comparison between them.

The three curves in the left-hand side of the figure represent the work of women workers. Metal-polishing is undoubtedly the hardest process of the three. It is a little difficult to say whether chocolate-packing or chocolate-dipping is the harder process, the former depending mainly upon such mental processes as discrimination and aesthetic judgment, the latter upon constant repetition of muscular effort. The graphs on the right-hand side of the figure represent the work of men in glass-blowing. It will be noticed that the graph representing the work of the old man shews well-marked indications of fatigue at the end of the day, whereas that representing the work of the young man conforms to the general contour of the graph representing the average of the twelve glass-blowers. The actual output of the old man was higher than that of the young man; this was particularly marked in the earlier part of the day, when his natural skill was allowed full play and was unaffected by the onset of fatigue as it was in the latter part of the day. This fact is, of course, not shown in the figure. It is interesting to note that the men's output curves vary far less than those of the women.

It is most difficult to obtain reliable output curves because, to be of real value, they should be taken by a skilled observer constantly watching the operations of the workers in question. If they are taken from the ordinary factory records, they are much less reliable for the purposes of fatigue study. For this reason each output curve that is obtained by a skilled observer is of considerable value to those who are studying fatigue. It cannot be expected that any one experimenter will be able to obtain a sufficient number of reliable output curves to afford information concerning the relative fatigue values of various operations. If, however, all of those working in industrial psychology who have the opportunity of getting reliable output curves would plot them on some uniform scale


Women
Men
Glass Blowers
Metal Polishing
Choc. Dipping
Packing
Dipping
Metal Polishing
Packing
Choc. Dipping
Packing
Dipping
Metal Polishing
Metal Polishing
Choc. Dipping
Packing
Young Man
Av. of 12 blowers
Old Man
Old Man
Young Man & Av. of 12 blowers
120
110
100
90
80
70
8.30-9.30
9.30-10.30
10.30-11.30
11.30-12.30
1.30-2.30
2.30-3.30
3.30-4.30
4.30-5.30
10 a.m.
11.15
12.30
3 p.m.
4.0
5.0

and send copies of them to some central bureau, it might be possible in time to arrive at the relative fatigue values of different industrial processes and also to determine what is a healthy and what is an unhealthy type of output curve. I believe that a very profitable line of research lies open along these lines.

3. USE OF THE STANDARD DEVIATION.

Another method of utilising output curves to which at present little attention has been paid is to disregard the gross amount of output at any particular period of the day and pay attention only to the standard deviation from the mean that each unit of work takes to perform at specified periods in the day. I am at present engaged on an experiment of this kind and the initial results suggest that it is at least worth continuing, though the experiment is not sufficiently advanced to be sure of its ultimate value. It is possible that by this means we may be able to determine how far various processes depend upon rhythm and how far upon voluntary effort and also to determine when rhythm is disturbed owing to the onset of fatigue.

4. CONCLUSIONS.

In conclusion I would like to emphasize the very important part the study of output curves must play in the study of fatigue. If an output curve indicates fatigue at all, it indicates, as has already been pointed out, genuine work-a-day fatigue, and not experimental fatigue caused by the performance of a task of exceptional severity specially designed to cause fatigue. In an output curve, adaptation and motor habit have done all that is possible to overcome fatigue. The special muscles called into play in a particular industrial operation have had time to respond to the special call put upon them; in fact everything that the human organism can do, unaided by special scientific knowledge, to ward off fatigue has already been done. Further, we may be fairly sure that, in most cases, the engineer and the employer have made the process as easy as possible, so far as it lies within their power to do so. Consequently anything further that can be done to relieve fatigue lies within the province of the physiologist or psychologist and can only be attained by means of careful research. In studying output curves, the scientific worker is studying a genuine and comparatively isolated problem, since they are very largely free from the interfering factors such as nervousness, desire to please, etc., which tend to affect ordinary performance tests. If he is able to effect a healthy change in the shape of an output curve, he can be sure that he has brought real relief to the worker and also achieved a definite advance in his own science.

THE CONSTANCY OF THE INTELLIGENCE QUOTIENT

By P. L. GRAY and R. E. MARSDEN.

1. *Object of the investigation* (p. 315).
2. *The tests used* (p. 315).
3. *Methods of testing and scoring* (p. 316).
4. *Selection of children* (p. 317).
5. *Conditions of testing* (p. 317).
6. *Change of I. Q.* (pp. 317–319).
7. *Change of I. Q. at the upper end of the scale* (p. 319).
8. *The repetition of individual tests* (p. 319).
9. *Personal equation of the examiner* (pp. 319, 320).
10. *Changes in the I. Q. group* (p. 320).
11. *Vocabulary test* (pp. 320, 321).
12. *Consideration of special cases* (pp. 321–324).

1. *Object of the Investigation.* If the Intelligence Quotient of a child is to have any diagnostic value, we must either know that it is constant within certain limits or determine the way in which it varies. Only a large number of re-testings of the same children at intervals during the period of mental growth can determine whether the I. Q. fluctuates or remains without change. As these re-testings must be spread over some years, time is necessary before the re-testings cover a sufficiently wide field to enable the formulation of laws of mental growth.

In chapter IX of *The Intelligence of School Children*, Terman gives the results of some investigations on this important point: the general conclusion appears to establish the approximate constancy of a child's I. Q., and thus to establish the I. Q. as a safe 'basis of prediction' in a child's educational progress.

The fundamental importance of the point prompted us to make a similar enquiry, the first step in which has been made by re-testing 100 children (50 boys and 50 girls) exactly one year after their I. Q.'s were first determined by means of the Stanford-Binet Intelligence Tests.

2. *The Tests Used.* The Stanford-Binet tests as described in Terman's *The Measurement of the Intelligence*, with the necessary modifications. (These are detailed in our paper "The Measurement of Intelligence in Some Elementary Schools," a paper not yet published.)

3. *Methods of Testing and Scoring.* In order to save time and to tire the children as little as possible, we decided that if a child answered five out of six tests in any one year on being first tested, it would only be necessary, in the second testing, to put the test in which the child failed and then pass on to the next year. In this way the weak places were tested and at the same time the method gave the children confidence, for when two tests were missed in any year, then the whole of the tests for that year were generally taken. It will be shown later how trustworthy this method proves in practice.

Among the factors that prevent the I. Q. from being mathematically constant in re-testings is the method of scoring. Very few even of the normal children answer all the tests up to those for a certain age and fail to answer the remainder, but instead spread their successful responses over a more extended range. A normal child of ten may fail on one or more of the tests for ten-year-old children but answer some at a higher age and so gain the normal score for ten years of age. Each test up to and including ten years of age counts for two months mental age if successfully passed, those for XII count for three months, for XIV a test adds four months, for XVI five months and for XVIII six months.

A child with an I. Q. 100 needs to add 12 months to his natural age in a year to get an I. Q. 100 the following year, while a child with an I. Q. 111 needs to add $\frac{111 \times 12}{100} = 13{\cdot}32$ months in a year to maintain a perfectly constant I. Q. As a child cannot score precisely this extra amount under any circumstances his I. Q. is bound to show at least a small variation.

To look at the matter in another way, suppose that a child in the first testing has Natural Age 144 months and Mental Age 160, then his I. Q. is $\frac{160 \times 100}{144} = 111$, and suppose that he has answered all the tests up to XII and four tests at XIV. Next year he can only increase his score by passing tests at XIV or higher age. If he passes three tests at XIV his mental age becomes 172 and his I. Q. $\frac{172 \times 100}{156} = 110$, or if he passes four extra tests at XIV it becomes $\frac{176 \times 100}{156} = 113$. This example shows the effect of the method of scoring on the I. Q. The effect can easily be greater in the case of those children who spread their scores over several years.

The point has been emphasized because the numerical value attached to the I. Q. has led many people to think it more accurate in itself than it can possibly be. One of the dangers of the use of figures in mental measurement is that they may give an appearance of exactness not warranted by the data on which they are based.

4. *Selection of Children.* The children re-tested were selected from the 224 children previously reported on, whom we tested in 1920. The selection covered the whole range of ability and chronological age, being made solely on the basis of the results of the intelligence tests and no reference was made to school work or progress. The 100 children selected can be taken as a true sample of the whole.

5. *Conditions of Testing.* In the preliminary testing we sometimes had the assistance of the head teachers; in the re-testing we carried out the whole of the tests ourselves, but arranged as far as possible that when a child was tested by one of us in the first testing then the other of us should test that child in the second testing. Consequently our results do in a great measure depend rather on the nature of the tests themselves than on the personality of the examiner.

The re-testing was done under the same conditions, and generally in the same rooms, as the original tests. Though it would have been easy to reject one or two results in which the difference of the two testings proved, on analysis, to be due to the fact that our first test was not carried far enough yet we considered it best to take the first hundred re-testings without any selection whatever.

6. *Change of I. Q.* In Table I are shown changes of I. Q. for all the children re-tested after one year's interval. These have been grouped (1) according to age at first test, (2) in the I. Q. group to which they belong, (3) according to the M. A. of the children at the first test.

It will be seen from a study of this table that there is no reason to believe that the N. A., I. Q. or M. A. influences in any way the changes that have taken place in the I. Q.'s after an interval of one year.

When our results are compared with those of Terman in 435 re-testings dealt with in *The Intelligence of School Children*, p. 142, it is found that there is a fairly close agreement between them.

(1) The central tendency of change is represented by an increase of 2·25. (Terman, decrease 2·3[1].)

(2) The middle 50 per cent. of change lies between the limits of 2·25 decrease and 7·7 increase. (Terman, 7·1 decrease and 3·8 increase[1].)

(3) The probable error (interquartile range) of a prediction based on the first test is 5·0 points in terms of I. Q. (Terman, 5·4[1].)

---

[1] These figures are slightly different from those given by Terman: they have been calculated by us from the table on p. 141 (*Intelligence of School Children*). Terman admits, in a letter to us, that some small arithmetical errors were made in working out the results of his Table 25.

Table I.

Showing I. Q. changes for children re-tested after an interval of one year, for children of different natural and mental ages, various degrees of brightness, and sex.

	N. A. at first test				I. Q. group					M. A.			Sex		
Changes in I. Q.	3–6 years	6–9 years	9–12 years	Above 12 years	Above 110 I. Q.	90–110 I. Q.	Below 90 I. Q.	Above 120 I. Q.	Below 80 I. Q.	48–95 months	96–131 months	132–191 months	Boys	Girls	No. of children
Above 20	1	—	1	—	1	1	—	—	—	1	1	—	1	1	2
16–20	1	—	2	—	3	—	—	—	—	1	—	2	3	—	3
11–15	—	3	2	1	2	4	—	1	—	3	1	2	2	4	6
6–10	1	9	10	1	3	10	8	1	1	8	10	3	13	8	21
1–5	—	7	12	5	5	14	5	3	1	6	10	8	13	11	24
0	1	—	5	1	1	5	1	1	—	1	4	2	3	4	7
-1–5	1	8	16	3	5	19	4	2	—	6	11	11	13	15	28
-6–10	—	4	—	—	1	3	—	1	—	1	2	1	—	4	4
-11–15	—	1	3	—	3	1	—	3	—	—	1	3	2	2	4
-16–20	—	1	—	—	1	—	—	—	—	—	1	—	—	1	1
Totals	5	33	51	11	25	57	18	12	2	27	41	32	50	50	100

For another comparison with Terman's results Table II has been drawn up; this shows the percentage of deviations in I. Q. greater than 5, 10, 15 and 20 points and also the theoretically determined amounts if the probable error is considered to be 5 points.

Table II.

Deviations of 100 *I. Q.'s.*

	Percentage		
Deviations as great as, or greater than	100 I. Q.'s	Terman (435)	Theoretical
5	50	50	50
10	19	16·6	27·76
15	6	6·2	4·3
40	3	1·85	0·7

It was shown in Table I that neither N. A., I. Q. group, nor M. A. appears to have an influence on the change of I. Q. in any marked manner.

It is interesting to note that when the boys are compared with the girls it is found that the boys are responsible for more increases of I. Q. and the girls for more decreases. This is shown in Table III.

Table III.

Change in I. Q. according to sex.

Sex	Increase	No change	Decrease	Total
Boys	32	3	15	50
Girls	24	4	22	50

The average change of I. Q. of the 100 children was + 2·6 (Terman, – 1·53) while the average change of the boys was an increase of 3·8 and the girls an increase of 1·4. The median change of the boys was an increase of 3·0 and of the girls 0·0.

7. *Changes of I. Q. at the upper end of the Scale.* Though it does not affect our results, yet it is well to remember that, as Terman points out, "the I. Q. as determined by the Stanford-Binet (or any other Intelligence scale yet devised) cannot *indefinitely* maintain its constancy in the case of children who are exceptionally superior. The child of fourteen years who tests at 139 has passed all the tests in the scale. Thereafter his I. Q. drops gradually to 122, which is the maximum possible for a subject of sixteen years who passes all the Stanford-Binet tests. Similarly, the child who tests at 161 has reached his maximum I. Q. at the chronological age of twelve years. This does not mean that his development ceases at this time, but merely that the Stanford-Binet does not measure it." The lack of tests beyond those given by the Stanford-Binet scale may mean that a child fails to reap the advantage of the possession of a high degree of ability in some special direction. At the ages over which elementary school children range there is always a possibility that a child may score in some directions much higher than in others. It will be seen, for example, that some of the 100 children re-tested showed a considerable ability for remembering digits.

8. *The Repetition of Individual Tests.* It was found necessary in order to get reliable figures to repeat 1572 tests given at the first testing. Of these 665 had been successfully passed in the previous year. These 665 tests were those the children found it hardest to pass and on which they were most likely to fail in the re-testings unless the ability represented by the previous successful answers remained constant. Out of these 665 tests 602 were again passed, or 90·5 per cent. This shows a high probability that any test passed successfully in one year will be passed the following year.

9. *Personal Equation of the Examiner.* It is obvious that for results to be trustworthy, the figures obtained "by different examiners in

testing the same subjects" should show close agreement. The following figures show a very satisfactory accord:

Of the 100 children re-tested, 77 were examined by different people on the two occasions. The average change in the I. Q. of these 77 was 2·1.

Nine children were tested on both occasions by G, average change, 3·8, and 14 children by M, average change, 3·1.

In considering these figures it must be remembered that a difference of not more than 5 points in the I. Q.'s means practical equality.

10. *Changes in the I. Q. Group.* It is sometimes more convenient to consider the children as grouped in the five classes suggested by Terman (very inferior, inferior, normal or average, superior, very superior), than it is to try to bear in mind the exact I. Q. of each child.

Our results show that only 17 changes of 'class' are made by the changes in I. Q. Seven of these changes are concerned with children on the border line between two classes in at least one of the testings and so, remembering the arbitrary nature of the divisions, need not be considered important changes. This leaves only ten important changes of class out of 100 children. Of these ten children only three have moved to a lower class. No great injustice would have resulted if the expectation of school work had been based on the results of the first testing. As will be seen from the notes on individual children, three of these ten children were not tested over a sufficiently wide range in the first testing to make the apparent change of I. Q. quite certain. From a school point of view the fact that at least 90 per cent. of the children are correctly classified by the first testing is very satisfactory.

11. *Vocabulary Test.* Of the 100 re-tested children 91 repeated the vocabulary test; the earliest application of the test is in VIII. Table IV shows the changes that took place in the scores, the average score, and the median score for sub-normal, normal, and super-normal children. Each column is treated separately, the total changes are given for the two columns added together; only 34 children were tested with both columns in both testings. In the vocabulary test the score consists of the total number of words known; half scores are allowed under certain circumstances. To save space any whole and a half has been included under the column for that whole number, but the half scores have been added in obtaining the averages.

The tables show that the average number of words added to the known words during the year increases with the intelligence of the children.

Table IV.

Changes in the Vocabulary Score during one year.

	I. Q.	No.	Below −5	−5	−4	−3	−2	−1	0	1	2	3	4	5	6	7	8	9	10	Above 10	Mn	Av.
First column	Below 90	17	—	—	—	1	2	—	—	1	3	2	2	1	2	1	2	—	—	—	3	3·3
	90–109	51	—	1	1	1	2	2	1	2	6	12	5	6	4	5	2	—	1	—	3	3·3
	Above 110	23	—	—	—	—	1	—	4	3	2	—	2	5	1	1	1	2	1	—	4	4·0
	Total	91	—	1	1	2	5	2	5	6	11	14	9	12	7	7	5	2	2	—	3·9	3·5
Second column	Below 90	6	—	—	—	1	—	—	1	1	2	1	—	—	—	—	—	—	—	—	1·5	1·1
	90–109	19	—	1	—	—	1	—	2	2	3	1	1	4	1	1	1	1	—	—	3	3·1
	Above 110	9	—	—	—	—	—	—	—	3	—	2	—	2	2	—	—	—	—	—	3	3·7
	Total	34	—	1	—	1	1	—	3	6	5	4	1	6	3	1	1	1	—	—	2·5	3·0
Two columns	Below 90	6	1	—	—	1	—	1	—	—	—	—	—	1	1	—	—	—	1	1	6	3
	90–110	19	1	—	1	1	—	—	—	1	—	—	1	1	1	2	2	2	1	5	8	7
	Above 110	9	—	—	—	—	—	—	—	—	—	2	—	2	—	—	1	—	2	1	8	8
	Total	34	2	—	1	2	—	1	—	1	—	2	1	4	2	2	3	2	4	7	7	6

NOTE. The vocabulary test consists of two columns of words which the child is asked to define. The left-hand column is called the first column. Mn=Median, Av.=Average.

12. *Consideration of Special Cases: Children who showed an I. Q. change of* 10 *or more points.* It is interesting to consider the scores of some of the children who showed an I. Q. change of 10 points or more. To save space the N. A., M. A., and I. Q. of each child will be given in the form $\frac{\text{M. A.}}{\text{N. A.}}$ = I. Q. in each test.

R. D. $\frac{54}{58} = 93$; $\frac{86}{70} = 123$. Increase of I. Q. 30 points.

This girl scatters her successes, and answered in the second testing, 5 tests at VI, 3 at VII, 3 at VIII and 2 at IX. At the first testing she answered only one test for VI and the testing was not carried further. The test in which she was successful was the repetition of 16 to 18 syllables. In the light of further experience it now seems desirable to exhaust the child's memory for syllables and digits in every case by going on with the tests of this nature till the child fails. This point is illustrated by other children below.

H. C. $\frac{122}{111} = 110$; $\frac{162}{123} = 132$. Increase 22.

This boy proves to have a good memory for figures and makes an extra score of 21 months mental age by passing tests in remembering digits alone. In addition he solved XIV–5 (Arithmetical reasoning) and XVIII–6 (Ingenuity test). The memory for digits and the arithmetical side of his ability were not fully explored on the first occasion as the testing stopped when he failed to answer more than one test in a year and this happened to be the repetition of 5 digits reversed.

A. P. $\frac{157}{139} = 113$; $\frac{201}{151} = 133$. Increase 20.

This boy failed to pass XII–6 (Repetition of 5 digits backwards) in the first testing, but was successful in this test in the second testing. In addition he passed XVI–5 (Repetitions of 6 digits backwards) and XVIII–3 (Repetition of 8 digits). These two tests alone added 14 months to his M. A. He also passed XVI–4 (Enclosed boxes), XVIII–2 (Binet's paper cutting test), and XVIII–4 (Thoughts of passage heard). These last three tests added another 17 months to his M. A. His memory drawing in X–3 showed great improvement and he also added XII–8 (Similarities) and XIV–4 (Problems of fact) to his score and increased his vocabulary from 47 to 63 words.

H. M. $\frac{50}{42} = 119$; $\frac{74}{54} = 137$. Increase 18.

This appears to be another instance where the first testing did not extend over a sufficiently wide range. He had not long been at school when the first testing took place. He stands out among his family for his intelligence.

L. T. $\frac{138}{119} = 116$; $\frac{173}{131} = 132$. Increase 16.

This boy adds 15 months to his M. A. in the second testing by passing XIV Alternative (Memory of 7 digits), XVI–5 (6 digits reversed), XVIII–3 (Memory of 8 digits) and XVIII–5 (Memory of 7 digits reversed). He failed in XVI Alternative in the first testing and thus appears to show a remarkable growth for the memory of digits during the one year interval between the testings.

L. W. $\frac{152}{120} = 126$; $\frac{184}{132} = 140$. Increase 14.

This boy increased his score by extending his vocabulary from 58 to 69 words. The extra tests passed seem to show a general growth of intelligence. He remains in the very superior class.

G. F. $\frac{94}{86} = 109$; $\frac{117}{98} = 120$. Increase 11.

This boy spreads his new successes over a wide range. He passes X–2, 3, 6, XII–6 and XIV–6 (Clock). His memory for digits should have been fully explored in the first testing.

I. Q. $\frac{130}{127} = 102$; $\frac{157}{139} = 113$. Increase 11.

Though this girl failed in XII–6 (Repetition of 5 digits backwards) in the first testing, yet she passed this in the second testing and in addition XVI–5 (Repetition of 6 digits backwards) thus gaining eight months M. A. She also passed XIV–5 (Arithmetical reasoning), XIV–6 (Clock) and XVI–4 (Enclosed boxes). These added another 13 months to her M. A. In addition she scored on the Ball and Field problem and on XII–7 (Pictures).

M. C. $\frac{84}{86} = 97$; $\frac{106}{98} = 108$. Increase 11.

On examination of the new successes in the second testing it was found that in the first testing there were five half scores which are never counted under the system of scoring. If these had been counted, then the I. Q. would have been 104. This girl remains in the Normal group.

J. A. $\frac{84}{82} = 102$; $\frac{106}{94} = 112$. Increase 10.

Exceptional growth appears to be the only explanation of the difference in I. Q.

All the children dealt with so far have shown an increased I. Q. in the second testing. Those below show a decreased I. Q.

H. K. $\frac{107}{92} = 116$; $\frac{102}{104} = 98$. Decrease 18.

This girl failed to pass in the second testing five tests passed in the first and yet passed three new tests. In failing to pass a second time in so many tests she is remarkable among the 100 children re-tested.

M. G. $\frac{182}{120} = 152$; $\frac{184}{132} = 140$. Decrease 12.

This is the most intelligent girl in our 100 children. All her work in school confirms the high I. Q. of the tests. She failed in X–3 (Memory drawing), in the first test, but passed easily in the second. She failed both times with the Ball and Field test for XII. In the second testing she failed on XVI–A 1 (Memory of 28 syllables), XIV–A 1 (7 digits) and XVIII–3 (8 digits) although she passed them the year before. This meant a loss of 15 months to her M. A.; if subtracted from her first score this would reduce her I. Q. to 139. As it is she remains in the same group.

E. F. $\frac{127}{142} = 90$; $\frac{120}{154} = 78$. Decrease 12.

This girl failed to add to her score any new success and also failed to score in XII–2 (Abstract words) and XVI–A 1 (Memory of 7 digits) which she passed successfully on the first testing. Possibly this may be one of those children whose intelligence does not increase beyond the age at which she then was.

H. P. $\frac{144}{106} = 135$; $\frac{146}{118} = 124$. Decrease 11.

There were no part scores in the first testing, but the part scores in the second testing would, if added, make this boy's I. Q. 130.

(*Manuscript received* 2 *April*, 1922.)

PUBLICATIONS RECENTLY RECEIVED.

Harmonism and Conscious Evolution. By Sir CHARLES WALSTON. London: John Murray. 1922. Pp. xvi+463. 21*s.*

The main thesis of this work is the attempt to establish the primacy of aesthetic experience in life, in the life of nature as well as of man, in all those spheres of action and aspiration which we have come to distinguish as aesthetic, scientific, moral and practical; that "aesthetic perceptions, emotions and principles...are so primary and so elemental in the evolution and the activity of the human mind that ultimately Truth, Utility and Goodness must be referred back to them." It is the same ideal which inspired the attempts of Schiller to base his philosophical speculations upon the "Kunst-Idee"; which led Shelley to identify Truth and Morality with Beauty in his "Defence of Poetry"; which may be found in the attitude of Guyau; which—despite the author's opposition to Nietzsche—informed that poet-thinker's speculations; which, though in a wholly different context and form, is implicit in Croce's philosophy, and in general represents a point of view which consciously or unconsciously shapes the thoughts of most artists. The author's central principle is 'Harmony,' or, on the subjective side, the 'harmoniotropic instinct' of man which seeks satisfaction in all fields of thought, action and aspiration. The first part of the book is accordingly concerned with the establishment, in a general way, of this 'harmoniotropic' tendency "in organic and sentient life," its persistence in conscious endeavour and the "dominance of the aesthetic attitude of mind in cultured life." The second part deals with the detailed demonstration of this thesis in Epistemology, Art, Pragmatics (a term the author uses for all activities aiming at the 'useful'), Ethics and Politics (in which connexion he is also led to touch upon Religion).

There can be little doubt that the book considered as a whole would be improved by condensation. This applies especially to the lengthy quotations from the author's other works (in the second part) and to the extensive excursions into the history of art (principally of sculpture and painting) which occupy so large a place in the section dealing with Aesthetics and Art properly speaking.

A certain looseness of terminology, very often merely irritating, may occasionally be suspected of having betrayed the author into false or at least unsubstantiated arguments. To speak of an "asymmetrical succession of dots or lines" (p. 14) is of the former kind. Aesthetic discussion would be all the better for avoiding the metaphorical use of terms like symmetry, rhythm, harmony, etc., and to confine symmetry and asymmetry, for instance, to arrangements of *visible* objects in reference to a central line. Again, the argument that an asymmetrical body, by being placed at an increasing distance from the eye, loses its asymmetry until "finally becoming but a round spot, it will correspond to other round spots and will form a unit for further symmetrical combinations" (pp. 17–18) comes dangerously near to playing with words. Unfortunately, the very word 'harmony' which stands in the author's mind for the central conception of his whole construction, lends itself with particular ease to misuse, though it is precisely the many-sided meanings of it which recommend it to the author as a substitute for 'aesthetic' (pp. 31–32). Considering the weight which this conception has to bear in his subsequent constructions, considering also the frequently admirable expositions to which he is led, as for instance the passage concerning 'conviction' (in the chapter on 'Epistemology'), it would be worth while to re-consider at every step the validity of the term and to make sure that it always really corresponds to the reality it is designed to express.

For the sake of the many brilliant applications of the conception and the unquestionably admirable unity of view which the author takes up towards the most varied and diverse problems that perplex the modern mind, we would beg him to make certain of this central principle. Compared with this difficulty, smaller criticisms are of infinitely less importance or interest,—such as the prominence he gives, perhaps unintentionally, to the 'typical' in art; the difficulty ot distinguishing on his principle between the beautiful and the useful (despite his excellent treatment of Architecture in this connexion); or his view of the evolution of the different mental attitudes described by the terms 'aesthetic,' 'scientific,' 'moral' and 'practical,' where he would seem to argue for a course of development, the opposite of that where he considers these values as differentiated with ever increasing purity in the course of human history.

Penal Discipline. By Dr MARY GORDON. London: George Routledge and Sons, Ltd. 1922. Pp. xiii + 238. 7*s.* 6*d.* net.

This is a disturbing book, as nearly all books about penal discipline written with real knowledge are apt to be. Dr Gordon has had many years of experience as an Inspector of Prisons, but writes not as an official but as a human being. The actual psychological applications are only a small part of her treatment, and are not in themselves very remarkable. But the book is one which psychologists ought to read. It is not merely critical, but constructive also. And in her attempts to point out necessary reforms of prison discipline Dr Gordon is dominated by the principle which she herself excellently expresses: "We should turn a fresh leaf in our treatment of the offender, fortified not by precedent, or by age-long prejudice, but by the findings of science, which is, at last, in the act of discovering the mechanism of the whole man." The style of the book is very lively and interesting, but is rather loose at times.

The Psychology of the Criminal. By Dr M. HAMBLIN SMITH. London: Methuen and Co., Ltd. 1922. Pp. vii + 182. 6*s.* net.

In many ways this is a first-rate little book. It is well-informed, accurate, and the fruit of much first-hand knowledge. At the same time it is frequently dull in its style, and it frequently leaves the reader wanting more. The introductory chapter on "What we mean by 'crimes' and 'criminals'" is admirably clear and concise. This is followed by a treatment of the methods of studying the offender's "conscious mind." Here a somewhat more critical attitude towards mental tests might well have been adopted. The bulk of the book deals with a study of the offender's "unconscious mind." This is clear, but rather conventional. Take it on the whole, and the volume may be warmly recommended as a good and judicious introduction to the psychology of the criminal.

Outwitting our Nerves. By Dr JOSEPHINE A. JACKSON and HELEN M. SALISBURY. London: Kegan Paul, Trench, Trübner and Co. 1922. Pp. 402. 7*s.* 6*d.* net.

This book comes from America. It is put forward as a "primer of psychotherapy" for "physicians who are eager for light on the subject of nervous disorders," but "who are too busy to devote time to highly technical volumes." At any rate nobody could possibly find this book "highly technical": its chapter headings are something like the sub-titles of a cinema film. There are, however, but pale and borrowed rays that stream from its pages.

The Emotion. Psychology Classics. Edited by KNIGHT DUNLAP. Vol. I. Baltimore, U.S.A.: Williams and Wilkins Co. $4.00 net.

This is a very excellently produced reprint of William James' essay on *What is an Emotion* (first printed in *Mind* in 1884), of the same writer's famous chapter on the emotion from the *Principles of Psychology,* and of Carl Lange's *Ueber Gemüthsbewegungen.* Lange's paper is well translated by Miss I. A. Haupt from Kurella's German version. It is undoubtedly a great convenience to have these three papers brought together in this easily accessible form, and both publishers and editor are to be congratulated upon their enterprise.

Modern Developments in Educational Practice. By Professor JOHN ADAMS. London: University of London Press. 1922. Pp. 302. 6*s.* net.

At a time when experiments in education are occupying the time and attention of so many experts, it is important that surveys of their work should be published to stimulate the interest of the public in a matter which concerns them so closely. Dr Adams has written such a book, which, on account of its simplicity and amusing style, is admirably fitted to serve such a purpose. He deals particularly with the dissatisfaction felt for the old Class System, and the compromises which have been made to avoid its worst evils, and to improve its results. This account is based on an admirable characterisation of the general modern attitude towards the relations of Teacher to Pupil, and the individual as pupil to the individual as a member of society.

Dreams and the Unconscious: An Introduction to the Study of Psycho-Analysis. By Professor C. W. VALENTINE. London: Christophers. 1921. Pp. 144. 4*s.* 6*d.* net.

This little volume is based upon a series of popular lectures; and forms an interesting endeavour to give, in a brief compass and in simple language, an introductory account of what the writer terms "the new psychology of the unconscious." The various chapters deal successively with mental conflict and repression, psychoanalysis through association and through dreams, the psychology of dreams, and the influence of the unconscious in everyday life. One of the most suggestive sections is that upon mental repression and educational discipline during childhood.

To the psychologist the chief interest of the book lies in Professor Valentine's attempt to give a systematic account of the views of the group of English psychologists whom he names 'neo-Freudians' (Rivers, Myers, McDougall, Hart, Crichton Miller, and Brown); and to show that the doctrine of psycho-analytic writers may be regarded as unfamiliar instances of familiar psychological principles, and so "brought into line with 'orthodox' psychology."

NOTES ON RECENT PERIODICALS.

Journal of Experimental Psychology, Vol. v.

No. 2, April 1922.

An Individual Curve of Learning—A Study of Typewriting. (Blanche M. Towne.)

The author studied her own curve of learning in the use of the typewriter. She found that the curve had no characteristic shape, that intense effort "consciously or unconsciously applied" was helpful, that one genuine plateau occurred, and that a pleasant feeling generally accompanied days of greatest achievement. The curve of errors was hyperbolic in form.

Effects of Loss of Sleep, II. (Edward S. Robinson and F. Richardson-Robinson.)

The Alpha Army Tests were given to a group of adults before and after a night without sleep, and a control group was used for comparison. The effects of loss of sleep appeared in a number of manifestations of disorganization which differed from individual to individual. The results of the tests gave no quantitative measure of these effects, probably because they were compensated by extra effort and interest in the members of the insomnia group.

Practice Effects in Intelligence Tests. (Edward L. Thorndike.)

The effects of practice in intelligence tests, where these are to be used for the study of fatigue, drugs, distraction and the like, are important. After a consideration of a very large number of experimental results, Thorndike concludes that practice has less effect in the case of more intelligent individuals; that ten minutes' fore-exercise is sufficient to reduce practice effects after the third trial to an almost negligible amount, and that certain tests, such as the reading of difficult paragraphs and answering difficult questions on them, are comparatively free from general or special practice effects.

The Conditioned Pupillary Reaction. (Hulsey Cason.)

This investigation was undertaken chiefly for its theoretical bearings, the importance of the pupillary reflex for the theory of Behaviourism having been pointed out by J. B. Watson in 1913. The author was able to condition the reflex to the sound of an electric bell, the buzzing of a telephone receiver, and an electric shock. A contraction of the pupil is more definitely conditioned than a dilatation, and the conditioning was readily established when the stimulus was given *during* a change in the size of the pupil. The author considers that the experiment "emphasises the reflex nature of the human organism."

The Effects of Repetitions Upon Retention. (Sugi Mibai.)

A repetition of certain of Ebbinghaus's experiments on Memory, which confirms his results.

No. 3, June 1922.

The Conditioned Eyelid Reaction. (Hulsey Cason.)

After training to a sound stimulus given *with* a shock stimulus, the time of winking when the sound is given alone may be so much reduced that the author concludes that voluntary factors are excluded, and this may be regarded as a true conditioned reflex.

An Instrument for Measuring Certain Aspects of Intelligence in Relation to Growth, Practice, Fatigue and Other Influences. (E. L. Thorndike.)

A precise estimate of the relative difficulty of the fifteen different forms of the thirty-minute test, Part I of the 1919–1923 Series of the Thorndike Intelligence Examination for High School Graduates.

Reliability of Scores in Steadiness Tests. (James Quinter Holsopple.)

An investigation of the conditions of use and the method of scoring which make the advantages of the telephone receiver, used in conjunction with the John Hopkins steadiness plate, greater than its disadvantages.

The Reliability of the Seashore Phonograph Record for the Measurement of Pitch Discrimination. (A. R. Gilliland and C. R. Jensen.)

A criticism of Seashore's use of unmounted forks for pitch discrimination, with a description of an apparatus in which the forks are mounted with brass resonators and struck by piano hammers. Comparative experimental results are given.

The Stimulus Error. (A. P. Weiss.)

A sound and careful reply to Fernberger's remarks on the "futility of properly interpreting the statistical results of a purely behaviouristic study without the control of the introspective report" (Fernberger, S. W., "An Experimental Study of the 'Stimulus Error,'" *J. Exper. Psychol.* 1921, IV, 63–76).

No. 4, August 1922.

Individual Variations in Retinal Sensitivity, and Their Correlations with Ophthalmologic Findings. (Percy W. Cobb.)

Measurements of retinal sensitivity made upon 101 subjects show individual differences far in excess of the accidental variations of the same individual and the same conditions; the distribution of these differences suggests a natural division of individuals into two groups, most numerous where the times of stimulation are 27 and 166 respectively. In general, no definite relation exists between retinal sensitivity and disqualification for flying status. An incidental conclusion is that binocular visual activity is dependent chiefly upon the eye which, used singly, shows the better vision, to the extent of nearly five times the dependence upon the poorer eye.

Adult Tests of the Stanford Revision Applied to University Faculty Members. (Helen Hubbert Caldwell.)

An investigation with the Stanford Adult Tests which leads the author to doubt the value of the I.Q. as now computed for adults, and to question the significance of the immediate memory tests in particular. The individuals tested were grouped according to whether their pursuits and attainments were scientific or literary, but the differences between the test results for the two groups were very slight.

Cumulative Correlations. (J. Crosby Chapman.)

A discussion of a method alternative to the partial correlation procedure which is considered to be much simpler, and employs relatively few equations.

Learning when Frequency and Recency Factors are Negative. (Joseph Peterson.)

An important and interesting account of experiments in mental maze learning with a method such that frequency and recency factors operate adversely. The influence of these factors is analysed quantitatively and in detail, the results substantiating the author's view that frequency and recency are entirely inadequate as explanatory principles for learning.

No. 5, October 1922.

Influence of Vision in Acquiring Skill. (H. A. Carr and Ella B. Osbourn.)

A study of the saving in number of trials and errors required to learn a maze pattern, when vision was used. Vision is found to reduce both trials and errors, but errors much more than trials. If the maze has been learnt without vision, the introduction of the latter brings about a varying degree of disturbance in some subjects.

Differences in the Oral Responses to Words of General and of Local Significance. (Vivienne R. McClatchy.)

Responses to stimulus words of local interest appear to show greater individual variations, both as regards the nature of the response and the reaction time, and to carry more feeling tone, than responses to words of a general character. There is some interesting 'individual' material.

The Cardio-Pneumo-Psychogram and its Use in the Study of the Emotions, with Practical Application.

A brief preliminary communication of an investigation of the value of cardio-respiratory changes as indices of emotion, particularly with regard to the detection of guilt. The cardio-pneumo-psychogram is held to be more reliable than association times, quantitative blood-pressure determinations or galvanometric methods.

A Study in Grades and Grading Under a Military System. (Robert L. Bates.)

A study relating to very special conditions, but with some significance for general problems of judgments of grading.

Studies in Dissociation. (Lee E. Travis.)

This paper deals mainly with changes in the auditory threshold induced by 'crystal gazing.' Changes occurred with eight out of ten subjects; of these individuals, four showed a raised, and four a lowered, threshold.

The Selectiveness of the Eye's Response to Wave-Length and its Change with Change of Intensity. (I. A. Haupt.)

In this investigation it was found that at the threshold the eye is most sensitive to colour in the following order: *A*, with a dark surrounding field: green, yellow-green, blue, blue-green, red, yellow and orange; *B*, with a light surrounding field: blue-green, green, blue, yellow-green, red, yellow and orange. At the point of maximum saturation the order is: *A*, yellow-green, green, orange, blue, blue-green, red and yellow; and *B*, yellow-green, green, orange, blue-green, blue, red and yellow.

The Psychological Review, Vol. XXVIII.

No. 6, November 1921.

Association as a Fundamental Process of Objective Psychology. (J. R. Kantor.)

In this long paper Association is reinstated as a fundamental and universal mechanism, not, however, as being a "mentalistic property" or a statement of mental connexions, but as a definite objective fact of psychological behaviour. Association processes are concerned with the organization of actual stimuli-response situations.

Sensation, Imagination and Consciousness. (J. E. Boodin.)

A very suggestive and picturesque survey of the functions and inter-relations of sensation, imagination, emotion and consciousness, in the light of the recent work of Head, Rivers and Cannon. Consciousness and 'mind' are held to be entirely irrelevant to the study of the individual organism, where all the facts can be projected on to the plane of physiology. "It is only when we are concerned with expressive signs, with social relations, that the concept of mind becomes necessary."

Some Unusual Visual After-Effects. (Howard C. Warren.)

A description of some cases of long-delayed after-sensations and of vivid visualisations, with a discussion of their origin and of the relation of peripheral to central processes.

The Psychological Review, Vol. XXIX.

No. 1, January 1922.

Prolegomena to Psychology. (William McDougall.)

This is the opening chapter of a forthcoming book entitled "A General Introduction to Psychology." Much of the chapter is a reiteration of the point of view of the author's volume in the Home University Library, with its pregnant criticisms of 'ideas' and other historical confusions. The new matter is mainly directed to the definition of Prof. McDougall's relation to what he regards as 'extreme' behaviourism, and to Neo-Realism. The development of behaviourism in its most recent forms creates in Prof. McDougall a desire to disclaim his own earlier definition of psychology as the study of behaviour. The demarcation between psychology and physiology is still, however, suggested to be mainly the direction of attention; physiology as the science of the physical and chemical processes of *organs*, and psychology, that of the behaviour of the organism *as a whole*. Physiologists themselves, we fancy, would repudiate such a delimitation of their field as entirely arbitrary and unjustified.

A New Formula for Behaviourism. (Edward Chace Tolman.)

A suggestion for a behaviourism which is non-physiological, and, it is claimed, would be able to embrace within itself "not merely the results of mental tests, objective measurements of memory and animal psychology as such, but also all that was valid in the results of the older introspective psychology."

Intelligence and Behaviour. (A. A. Roback.)

A severe criticism of the various attempts that have been made by behaviourists to define intelligence in terms of behaviour.

The Meaning of the Term 'Practice.' (Georgina Strickland Gates.)

A useful discussion; the author finds, in her survey of the use of this term in current literature, at least four distinguishable meanings. The commonest obscurity of meaning is as to whether the term refers to mere repetition or to the resulting improvement, when such occurs.

Conscious Analysis in Learning. (H. R. Crosland.)

A vigorous attack upon the tentative views as to the function of consciousness in the learning process put forward by F. A. C. Perrin ("Conscious Analysis *versus* Habit Hierarchies in the Learning Process," *Journ. of Compar. Psychol.* 1921, I. 287–308), and upon behaviourist assumptions in general.

No. 2, March 1922.

Introspection as an Objective Method. (Margaret Floy Washburn.)

A temperate and just defence of the introspective method, reminding those who reject it how large a part it has contributed and continues to add to behaviouristic facts themselves, and, indeed, even to neurology and physiology.

The Crux of the Psychological Problem. (J. A. Melrose.)

The central problem of psychology is considered to be the problem of conscious 'thinking' reflexion, and distinctively human behaviour is the "adaptive use of a vocal sign system known as language." There is a discussion of the Oxford symposium on "Is Thinking Merely the Action of Language Mechanism?"

A Physiological-Genetic Theory of Feeling and Emotion. (Floyd H. Allport.)

The James-Lange theory is held to have "retained the merited esteem of psychologists," its defect being that it fails to differentiate the patterns of visceral and somatic response. The present paper attempts to formulate a theory for differentiating the emotions physiologically.

Concerning the Sensation Quality—A Behaviouristic Account. (Edward Chace Tolman.)

It is argued that whenever a person senses a sensation-quality there is (1) a "raw feel" or *quale* directly before his mind, and there is (2), at the same time, a readiness of the corresponding 'term-character' of the quale to function in his behaviour. And "all that gets in to our psychology, whether we call it behaviourism or whether we call it introspectionism, is not the qualia, themselves, but merely their term-characters, *i.e.* the *behaviour potentialities* of those qualia."

The Stimulus-Response Relation. (James L. Mursell.)

The position is taken up that both behaviour-psychology and self-psychology are in the last analysis working hypotheses, and "there seems nothing to prevent their proponents from living in as much co-operative harmony as euclidean and non-euclidean geometers."

Rivista di Psicologia, Vol. XVIII.

No. 1, January–March 1922.

Imagination as an Autonomous Psychical Activity. (F. de Sarlo.)

Imagination, though not primary, is autonomous, and neither play nor art, which are typical forms of imaginative activity, can be reduced to any form or explained by any laws other than those which constitute and govern this activity. This means, of course, that the individual work of art is not, as some have held, a law to itself, but is subject to the general laws of imagination.

A Veridical Prophetic Dream. (G. C. Ferrari.)

Report of a dream, held to be well-authenticated, correctly predicting in August, 1921, the death of the Pope "shortly between the 20th and 25th January" and the name of his successor.

The Centres of Emotive Reflexes. (G. Pellacani.)

A general survey of the functions of the various brain centres in the interest of the thesis that all emotion is simply the subjective counterpart of a purely physical nervous commotion; in harmony with the general postulate of psycho-physical identity.

Psychological Monographs, No. 140, 1922.
University of Iowa Studies in Psychology, No. VIII.

This volume contains seventeen articles, mostly extended abstracts of longer papers, which have accumulated owing to post-war conditions of printing.

The first three articles deal with the wave-phase localisation of sound. Prof. Seashore, in his introductory article, explains the laboratory problems on which they were engaged in 1917 and summarises the facts as observed up to December 1918. Dr Halverson describes the experiments he performed to determine "the role of intensity in auditory wave phase." Unlike many previous experimenters, he found that the effects could best be studied without the aid of conductors to the ear. He used stationary waves, produced by a tone generator (a Leeds and Northrup oscillator) connected up with two telephone receivers placed at a convenient distance apart, and traced the course of the phantom sound obtained as the head is moved between the receivers. He also compared the intensities of the sound in the various parts of the wave as observed introspectively and as measured by a Seashore audiometer, and found general agreement between the two. A variation of intensity in the sources produced no change in the localisation of nodes and loops; but when the two receivers were pressed close to the ears a movement of the phantom was observed. This monograph is not complete. It is understood that the principal results of these and of later experiments have been published in the *Amer. Journ. Psychol.* vol. XXXII. Prof. Stewart's article on "The Intensity Logarithmic Law and the Difference of Phase Effect in Binaural Audition" contains further data said to substantiate the law that the 'Intensity Effect,' *i.e.* the apparent displacement of a fused sound from the median plane when the intensities of the sounds at the ears, I_r and I_l, vary, is given by the formula

$$\theta = K \log_e \frac{I_r}{I_l},$$

where K is a constant for a given individual, varying however with the frequency of the sound. He found also that there are certain frequencies, depending on the individual, for which the Intensity Effect is absent. From other experiments he concludes that the 'Phase Effect,' *i.e.* the alteration of the angular displacement from the median plane of the apparent source of the fused sound caused by varying the phase differences at the two ears is proportional to the frequency of the pure tone employed. It was practically constant for the three persons experimented on. The effect vanished, however, at high frequencies. Stewart, who used a tuning fork as his source of sound, the sound being led to the ears by stethoscope binaurals, considers that the phase effect cannot be explained in terms of intensity, and that the organs of hearing must respond to phase as such.

The next two articles describe in detail new models of the Iowa Pitch Range Audiometer, and their utility in measuring acuity of hearing.

Of the remaining articles eight deal primarily with problems connected with the psychology of music, viz. Visual training of the pitch of the voice; the pitch factor in artistic singing; voluntary control of intensity of sound; voice inflexion; auditory imagery; inheritance of specific musical capacity; and a survey of musical talent in a music school. The other four papers describe a pursuit apparatus to test coordination between eye and hand in following a mark revolving at a constant speed; the use of Serial Action and the Tapping Test, for both of which new apparatus has been designed, as a measure of motility; and an inexpensive device for measuring rotation rates.

American Journal of Psychology.

Vol. XXXII, No. 3.

An Experimental Investigation of the Positive After-Image in Audition. (H. G. Bishop.)

There is no positive after-image of tone. Tones have a "modified ending" due in part to '*Abklingen,*' and in part to objective conditions. Attempts to determine whether air or electrical conduction of sound is to be preferred failed to produce any definite conclusion.

The Influence of Color upon Mental and Motor Efficiency. (S. L. Pressey.)

Hue is not an important factor in determining efficiency. Possibly dim light slows mental work, and bright light stimulates it.

The Nature of the Affective Judgment in the Method of Paired Comparisons. (M. Yokoyama.)

So far as investigation with the method of paired comparison goes, pleasure and unpleasure seem to be "statable as meanings," and on the whole appear as sensory in character.

A Study in Logical Memory. (Sarah D. M. Austin.)

With logical material divided repetitions within limits are more effective as a method of learning than cumulative repetition, but this does not hold good for immediate recall.

In Aid of Introspection. (H. B. English.)

Seven rules are given to aid the process of introspection.

The Minor Studies from Cornell University are: An Experimental Study of Cutaneous Imagery (Catherine Braddock); The Integration of Punctiform Cold and Pressure (S. Tang); The Hering Colour-Blindness Apparatus and the Normal Rayleigh Equation (M. Winfield and C. Strong); and The After-Effect of Sun Movement when the whole Visual field is filled by a Moving Stimulus (W. A. Thalman).

Vol. XXXII, No. 4.

The Stimulus-Error. (E. G. Boring.)

Recent researches have shown that "the observational attitude which is directed upon the stimulus—the attitude of the stimulus-error—may sometimes lead to equivocal correlations of stimulus and response which, because equivocal, are unscientific."

The Illusory Perception of Movement on the Skin. (Anna K. Whitchurch.)

A study of the conditions under which two separate and successive stimulations may give rise to a perception of movement over a cutaneous area.

Some Qualitative Aspects of Bi-tonal Complexes. (C. C. Pratt.)

A careful analysis of the nature of the complexity of the tonal impression that is produced by the presentation of bitonal stimuli.

On Arterial Expansion. (G. N. Hautman and D. L. McDonough.)

A method of dealing with volumetric peripheral vascular changes in a quantitative way, by means of a combination of the sphygmomanometer and the plethysmograph.

Functional Psychology and the Psychology of Act. (E. B. Titchener.)

Functional systems of psychology have four main characteristics: "The subject matter of psychology is duplicated, though function is preferred to content; consciousness is a solver of problems; the whole course of the mental life is regarded teleologically; and psychology is written as a preface to philosophy or to some practical discipline."

Church History and Psychology of Religion. (P. Butler.)

Historians and psychologists should work together. The conclusions of the historian should be tested by their conformity to psychological law; while the psychologist's researches should be directed by the historian's knowledge.

Death-Psychology of Historical Personages. (A. MacDonald.)

An analysis of the last sayings of important people.

The Minor Studies from Yale are: An Experiment in Time Estimation, using Different Interpolations (J. E. Anderson); The Involuntary response to Pleasantness (G. H. Corwin); and The Integration of Punctiform Warmth and Pressure (R. S. Malmud).

Arbeiten zur Entwicklungpsychologie.
1 Band, Heft 2.

Ueber die Vorstellungen der Tiere. (H. VOLKELT.)

This monograph drives another nail into the Associationist coffin. Herr Volkelt believes that the perception mechanism of animals is "formally and qualitatively" different from that of human beings, though it is the mechanism from which the latter has evolved. To modern English psychological students the general thesis of the book is familiar, but seldom is the "undifferentiated background" so carefully and clearly dealt with. The author applies a theory of Form to Animal psychology, which places his work in close relationship with that of Thorndike on the one hand, and the mass of important work on 'Gestaltqualitat' which is being done in Germany. The animal is affected by the 'Formquality' of a whole experience, rather than by individual constituents.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETINGS.

November 11th, 1922.	A Case of Word Deafness, by L. G. Fildes.
	The Influence of Subjective Preference on Memory, by Charles Fox.
December 16th, 1922.	The Analysis of the Work Curve, by S. J. F. Philpott.
	Hand and Ear Tests, by Hugh Gordon.

SECTIONAL MEETINGS.

Education Section.

October 10th, 1922.	The Problem of Time-Sense in the Teaching of History, by M. Sturt and E. Oakden.
November 7th, 1922.	The Synthesis of Geographical Information, by E. J. G. Bradford.
December 5th, 1922.	The Memory of Feeble-minded Children, by E. O. Lewis.

Medical Section.

October 25th, 1922.	Some Medical Novelties in Vienna and the Nature and Treatment of Epilepsy, by E. W. Scripture.
November 29th, 1922.	The Importance of the Instinct of Self-Preservation, by E. N. Snowden.
December 20th, 1922.	An Attempt to Explain the 'Reality-Feeling' associated with the Phantasies of the Insane, by H. Devine.

McDougall discovers also only one pair of names for experiences radically distinct.

A concluding paragraph may serve to suggest a second possible reason for McDougall's confusion of elation with self-assertion and of shame with submission. This reason lies in the fact that volition plays for the most part so slight and so equivocal a rôle in his psychology. *An Introduction to Social Psychology* seems, in truth, to embody two views of will. On the one hand, as earlier quotations have indicated, will is treated as self-activity, a characteristic attitude of the subject. But far more often volition is conceived simply as a condition of bodily reaction (or of attentive apprehension)—in a word, as "only a specially complex case of conation[1]" (conation including "all impulses, desires, aversions, motives"). The formal definition of volition[2] as "the supporting or re-enforcing of a desire or conation by the co-operation of an impulse excited within the system of the self-regarding sentiment" is of this type and (it will be noted) is in more or less ideistic terms. The confusion of the emotions, pride and shame, with those active experiences, self-assertion and submission, may well then be due in part to McDougall's general tendency to co-ordinate volition with emotion (or even to reduce volition to emotion)—a tendency responsible, more recently, for his treatment of belief, so obviously active and 'voluntary' an experience, as a kind of emotion[3].

---

[1] *Ibid.* ch. IX. 237.

[2] *Ibid.* ch. IX. 249.

[3] "Belief as a Derived Emotion," *Psychol. Rev.* 1921, XXVIII. 315 ff. McDougall, however, in the course of this paper, says that belief is "experienced only in the course of the operation of desire or volition" (p. 321), and frequently suggests the true relation of emotion to belief—namely that emotion, while distinct from belief, leads to, and excites, or accompanies it. His confusion of the two seems to be due, in part, to his implicit adoption of the antiquated classification of psychic phenomena into the two groups, cognitive and conative. For this classification necessitates the uncritical association of volition and emotion under one head.

⁂ A final word remains. This paper was written and submitted for publication in this *Journal* several months before the issue of Professor McDougall's latest book. It may best be described now as an appeal from McDougall, writer of *An Introduction to Social Psychology*, to McDougall, writer of the *Outline of Psychology*. For this most recent book is free, or all but free, of ideistic phraseology, and neither confuses self-assertion with self-display nor submission with shame. Professor McDougall still retains, to be sure, the view of conation which I have ventured to question; and, though he describes the different types of experience in terms both of subject and object, he still seems to me to ignore certain distinctions in attitude of subject to object which are very relevant to psychological description. I am, however, most concerned to stress the notable advance of the *Outline of Psychology* upon the *Social Psychology* and to emphasize the signal contribution of McDougall, in this latest book, to the doctrine of psychology as science of the experiencing mind, or self.

(*Manuscript received* 16 *January* 1923.)

VOCATIONAL TESTS AND TYPEWRITING

By B. MUSCIO, M.A.

Assisted by S. C. M. SOWTON.

(A Report to the Industrial Fatigue Research Board.)

I. *Aim of investigations here reported* (pp. 344–347).
II. *Previous investigations into vocational tests for typists* (pp. 347–349).
III. *General description of investigations here reported* (pp. 319–353).
  (*a*) *Persons to whom tests were given.*
  (*b*) *The tests used in the investigations.*
  (*c*) *Method of the investigations.*
IV. *Results* (pp. 353–359).
V. *Discussion* (pp. 359–368).
VI. *Conclusions* (pp. 368–369).

I. AIM OF INVESTIGATIONS HERE REPORTED.

In vocational selection investigations the usual procedure has been to give selected physiological or psychological tests to a relatively small group of persons engaged in the occupation being studied, and to compare the relative efficiency of these persons in their work with their relative efficiency in the tests; those tests then being regarded as useful for selecting workers (for the given occupation) in which the relative efficiency of the persons tested shewed a general correspondence with their relative efficiency in the given occupation. This procedure involves assumptions and serious difficulties.

The method assumes that relative efficiency in an occupation is a measure of relative capacity for it, at least when certain factors, the presence of which would obviously make this assumption false, are eliminated. Thus, different degrees of experience or of training in the occupation is a factor that would render the assumption false; if one typist, for instance, had had five years' experience of typewriting and another only one, we could infer nothing as to the relative typewriting *capacities* of the two from the fact (supposing it to be a fact) that the former had the greater speed. Consequently, investigators (who have generally been alive to this difficulty) have endeavoured to obtain for experimental purposes groups of workers whose members had all had about the same amount of experience of the work; and this is sometimes fairly well guaranteed if they are of about the same age. But in the usual industrial concern it is impossible to obtain such a group of any considerable size; and it is chiefly owing to this fact that the groups tested in vocational selection experiments have as a rule been small.

With such groups, engaged in a variety of occupations, a certain amount of experimentation has already been carried out[1]. In a few occupations a *number* of investigations has been made, each investigator dealing with a *single* small group of industrial subjects. Now, when the results of several such investigations, made by different investigators into the one occupation, are brought together, they are often seen to be in more or less disagreement. It is the case sometimes, perhaps, that they are merely complementary; single investigations have not been exhaustive, and the tests used by any one investigator have been concerned with only a part of the capacity required for the occupation. If another investigator obtains similar results with other and different tests, that means, perhaps, that his tests are concerned with the other part of the required capacity. Had investigator *A* used the tests employed by investigator *B* in addition to those he did use, he might have obtained results in perfect agreement with *B*'s, and seen that *all* the tests were required for satisfactory vocational guidance in the occupation concerned. But this is not the case when the results of different investigators are in definite disagreement; a difficulty made still more clear when some one investigator obtains results with one group of subjects which do not agree with his own results obtained from another group.

Such a phenomenon may be illustrated by certain investigations into vocational tests for salesmanship. Münsterberg[2] gave a number of 'intelligence' tests to two groups of salesmen in different stores, one group consisting of forty, the other of seventy, persons. Each group was divided into efficient and inefficient salesmen. With the former group, the efficient salesmen did no better in any of the tests than the inefficient, but with the latter group, performance in the tests corresponded more or less with selling efficiency. Why did not performance in these tests correspond with selling efficiency in the group of forty also? Again, Oschrin[3] gives a number of positive correlation coefficients obtained between mental tests and salesmanship ability, the subjects (18 in number) being engaged in selling in a department store. The three tests that shewed the greatest correspondence with salesmanship efficiency were 'Opposites,' 'Mixed Relations,' and 'Re-arrangement of Animals,'—all 'intelligence' tests—the coefficients of correlation of these tests and efficiency in selling being respectively + ·65, + ·50, + ·45[1]. The investigator concluded that these tests would be useful diagnostic instruments of salesmanship ability of the lower grades. But in a more recent paper[2], the same investigator presents results which are entirely at variance with these. The above three tests, along with a number of others, were given in the later investigation to a group of forty-eight persons engaged as sellers in a department store (the tests all being 'intelligence' tests). The results shewed a clear *inverse* correspondence between selling efficiency and efficiency in the tests; *i.e.* those who made the best performances in the tests were the poorest sellers and those who made the worst performances were the best. The three tests chosen in the earlier experiments as useful diagnostic instruments of salesmanship ability of the lower grades ('Opposites,' 'Mixed Relations,' and 'Re-arrangement of Animals') here yielded respectively correlations of − ·16, − ·37, and − ·56. From the earlier results, the *presence* of 'intelligence' appears to be useful for a salesman, from the latter, its *absence*!

---

[1] See the *Industrial Fatigue Research Board's Report*, No. 12, "Vocational Guidance" (A Review of Literature).

[2] H. E. Burt, "Professor Münsterberg's Psychological Tests," *Journal of Applied Psychology*, 1917, I. 209–211.

[3] E. Oschrin, "Vocational Tests for Retail Saleswomen," *ibid.* 1918, II. 148–155.

Explanations of such inconsistency in results might be offered. It might be said that the selling ability ot the subjects was much better known in one case than in the other; that the actual selling was different in the two cases; that the constitution and size of the two groups were different—for instance, the subjects in the group of eighteen were all women (apparently young), while in the group of forty-eight both sexes were represented, and "all ages from 17 to 60"; that the testing of one group was carried out under more favourable conditions than the testing of the other; that experience in selling was much more of an interfering factor in one group than in the other, and so on. But no one denies that the difference in results had a cause. The question is: what is the conclusion so far as the use of vocational tests for salesmanship is concerned? Shall a person be advised to become a salesman on the basis of the presence or on the basis of the absence of 'intelligence'?

One great difficulty in vocational test research concerns the reliability of an occupational efficiency grading. If an investigator into vocational guidance is to use an industrial efficiency grading of any group of workers, it is essential that this grading be a *real indication of relative capacity*. But normally it will not indicate this only, nor even primarily, and it will be influenced by different factors in different cases. (*Cf. Industrial Fatigue Research Board's Report*, No. 16, pp. 35–37.) This is perhaps the main explanation of the fact that one group of workers in a given occupation may shew positive correlation between their occupational and their test efficiency while another group in the same occupation may shew negative correlation with the very same tests. To mention but one aspect of the difficulty—that concerning age, and consequent experience, mental flexibility, and confidence:—is it to be expected, for instance, that an *occupational* efficiency grading of a group representative of all ages between 16 and 70 will indicate anything like *real* relative *capacity* for the occupation?

---

[1] See the *Industrial Fatigue Research Board's Report*, No. 12, pp. 35–36.

[2] E. Oschrin Bregman, "A Study in Industrial Psychology-Tests for Special Abilities," *Journal of Applied Psychology*, 1921, v. 127–151.

The groups that are required by the investigator into vocational guidance should be, as far as possible, homogeneous as regards (*a*) age, (*b*) experience of and training in the occupations concerned; and they should be heterogeneous as regards capacity for the occupations. They should be as homogeneous as possible in all other respects than occupational capacity; but age, and experience of the occupation, seem to be the essential of these other respects.

The aim of the investigations here reported may now be stated as follows: to determine how far it was possible to obtain suitable groups for vocational test research in the occupation of typewriting, and, if such groups could be obtained, to attempt to arrive at vocational tests for this occupation. The general method was to obtain various groups of typists, the groups being as suitable as could be found for the research purpose in question, and to determine whether the same tests gave the same or different results with different groups.

II. PREVIOUS INVESTIGATIONS[1] INTO VOCATIONAL TESTS FOR TYPISTS.

Lahy, having as subjects eleven typists, men and women, found considerable correspondence between typewriting efficiency and the following capacities: (1) Immediate Memory for concrete phrases; (2) Tactile Sensibility; (3) Muscular Sensibility; and (4) 'Muscular Symmetry' (tendency to equality in the strength of grip of the two hands). No such correspondence was shewn with certain other capacities, namely, attention, auditory reaction-time, immediate memory span for digits, and reasoning ('intelligence'). Lough obtained a general correspondence between ability to improve in a 'substitution' test (a test of memorising—the acquiring of associations) and typewriting capacity, the subjects being High School pupils (number not given). Lahy made use of no test similar to this substitution test, and, consequently, if the results of both investigators are valid, Lough discovered an aspect of typewriting capacity which escaped Lahy's tests. Rogers gave a number of mental tests to a group of forty-five young women students of typewriting, the tests falling under the following four heads: (*a*) 'intelligence' tests ('opposites,' 'mixed relations,' 'directions'); (*b*) 'attention' tests (cancellation); (*c*) 'memorising' (or acquiring of associations) tests ('substitution'); (*d*) 'speed of association' tests ('verb-object,' 'agent-action,' 'action-agent,' and 'colour-naming'). The results were correlated with the results for each of six monthly examinations in typewriting. In agreement with Lahy, no significant correspondence was found to obtain between 'intelligence' tests and typewriting efficiency; in disagreement with Lahy, a significant correspondence was found to obtain between typewriting efficiency and 'attention' (though the actual test material of the two investigators differed); in disagreement with Lough, the correspondence of the substitution test efficiency with typewriting efficiency, was barely significant (though here, too, the actual test material of the two investigators was different); and finally the tests yielding the greatest correspondence with typewriting efficiency were three of the 'speed of association' tests. Link, though he does not present details of his experiments, states that he found four tests of practical use in selecting typists. One of these was a 'substitution' test, as regards which his statement is in agreement with Lough's results and in disagreement with those of Rogers. Another was an 'intelligence' (*completion*) test, and as regards this his statement is in opposition to the conclusions of both Lahy and Rogers as to the relevance of 'intelligence' to typewriting, though neither Lahy nor Rogers used a 'completion' test. (His other two tests were tests of acquired ability—a test of typewriting itself, and a test of spelling—and these would naturally be of *practical* use to an employer engaging a typist[1].)

---

[1] These have been described in the *Industrial Fatigue Research Board's Report*, No. 12, pp. 13–16, and full references are given in this report.

Bringing the foregoing data together, we note the following interesting facts: the test of 'attention' (cancellation), the tests of the acquisition of associations (substitution), and the tests of 'intelligence,' have all yielded divergent results; one investigator obtaining a positive correspondence between typewriting efficiency and a given sort of test, and one or more investigators finding no such correspondence (though it is to be observed that the test material differed from one investigator to another). Indeed, the only tests with which a particular investigator obtained results so far uncontradicted are tests that have so far not been tried by other investigators. These are four of Lahy's tests (those for immediate memory for concrete phrases, tactile sensibility, muscular sensibility, and 'muscular symmetry'), three of those used by Rogers (verb-object, action-agent, and colour-naming), and two of Link's tests ('completion,' and spelling).

---

[1] It may be mentioned in connexion with the question of the intelligence level required for any given occupation, that American typists obtained a relatively high score in the Army Alpha tests (that is, compared with the scores of persons engaged in other occupations).

III. GENERAL DESCRIPTION OF INVESTIGATIONS HERE REPORTED.

(*a*) *Persons to whom tests were given.* From the point of view of the subjects, the present investigations fall naturally under the following six heads:

A. Three groups of young women typists in the employment of a large manufacturing firm. The groups consisted of 24, 20, and 18 respectively. They will be referred to as the A groups and will be designated A_1, A_2, and A_3.

B. A group of 22 young women typists engaged in a typewriting office which specialised in the typing of authors' MSS. This will be referred to as Group B.

C. Three groups of pupils at a large commercial school (both sexes, though mostly women). The groups consisted of 60, 22, and 30 respectively. These will be referred to as the C Groups and designated C_1, C_2, and C_3.

D. A group of 26 typewriting pupils (boys and girls) in the highest form of a L.C.C. Central School. These were treated in two sub-groups owing to the fact that some subjects had had more typewriting practice than others. They will be referred to as the D Groups and designated D_1 and D_2.

E. A group of 10 young women typists in the employment of a typewriting office. These will be referred to as Group E.

F. A group of 10 young women typists in the employment of a small business firm. These will be referred to as Group F.

Group A_2 included some of the subjects of A_1, and C_3 consisted entirely of subjects belonging to C_1. Altogether, tests were given to approximately 220 subjects all possessing more or less experience of typewriting.

(*b*) *The tests used in the investigations.* The choice of the tests made use of in these investigations was determined partly on the basis of earlier investigations, and partly as a result of suggestions derived from a theoretical analysis of operating a typewriter. They may be placed under the following eight heads:

(1) 'Attention' (probably more accurately 'rapidity of observation').
(2) 'Memorising'—acquiring associations.
(3) 'Immediate Memory Span.'
(4) 'Speed of Association' (more accurately 'speed of the reproduction of associations').
(5) 'Intelligence.'
(6) 'Tactile Sensibility.'
(7) 'Motor Capacity.'
(8) 'Acquired Associations' (knowledge).

The tests used were these:

Under (1):

(*a*) *Cancellation tests:* (α) a page of digits in which every 5 was to be cancelled; (β) the Woodworth and Wells *number group checking* test (cancelling every group containing both 2 and 9: this test is given in the *Industrial Fatigue Research Board's Report*, No. 16, p. 12); (γ) a page of senseless French in which every *e* was to be cancelled (given in above *Report*, p. 11);

(*b*) a *Finding test:* finding products of pairs of two-place numbers from a table (this test is given by Thorndike, *American Journal of Psychology*, 1916, XXVII. 533);

(*c*) a *Sensory Discrimination test* (writing S or D between pairs of numbers, pairs of groups of letters, names, and so on, according as the two members of a pair were identical or not).

Under (2):

(*d*) *Substitution tests:* (α) the Woodworth and Wells *form substitution* test; (β) a test in which the blank consisted of nine signs repeated in various orders, for each of which one of the digits from 1 to 9, had to be substituted; (γ) a *letter-digit substitution* test (this test is given in the *Industrial Fatigue Research Board's Report*, No. 16, p. 14).

Under (3):

(*e*) Immediate reproduction of *sentences* after a short exposure (the material and method identical with that described in the *Industrial Fatigue Research Board's Report*, No. 16, p. 15);

(*f*) Immediate reproduction of *series of digits* after a short exposure (this material was relatively non-significant—that is, compared with that in the immediately preceding test).

Under (4): the two Woodworth and Wells tests known as

(*g*) *verb-object;*

(*h*) *action-agent.*

Under (5):

(*i*) *Opposites tests:* (*α*) the Woodworth and Wells *Easy Opposites*; (*β*) the Trabue and Stockbridge *Graded Opposites* (given in *Measure your Mind*, by these authors, p. 196);

(*j*) *Trabue Completion test* (see M. R. Trabue's *Key for Completion Test Language Scales*. Teachers College, Columbia University, 1919);

(*k*) a *Directions test* (modelled on the Pintner and Toops modification of the Woodworth and Wells test; *cf.* the *Industrial Fatigue Research Board's Report*, No. 16, pp. 16–17, where a similar test is given);

(*l*) a *Discrimination of Meanings test* (subjects were required to write either S or D between pairs of words according as these words were the same or different in meaning);

(*m*) *Analysing a Table* (to obtain from it certain specified information. The nature of the test may be inferred from the first part of the instructions, which was: "In the column marked G put the figure 1 opposite the name of every girl in the list who is twelve years old or over, who has *passed* the 7th grade, and whose average marks are 80 per cent. or over." *Cf.* S. Cody, *Commercial Tests and how to use them*, pp. 58, 59);

(*n*) *The* (*American*) *Army Alpha tests;*

(*o*) *The Stanford-Binet* (*intelligence*) *test.*

Under (6):

(*p*) *Discriminating by Touch* (eyes closed) between playing cards having two, three, or four small holes punched in them close together and near the centre of the cards. The subject held a card by a corner with his left hand, and by moving the ball of the thumb of his right hand over the hole area on the upper side of the card, and the corresponding parts of the first and second fingers of the same hand over the same area on the under side of the card, decided whether a card contained two, three, or four holes. The score used was the time taken to make judgments about all the cards (of which there were forty) modified for errors. This tests is here called *Touch Cards.*

Under (7):

(*q*) *Tendency to equivalence in strength of grip of right and left hand* (tested by the Smedley hand dynamometer; *cf.* the *Industrial Fatigue Research Board's Report*, No. 16, p. 43);

(*r*) *Match Stick Insertion test* (putting match sticks one at a time into small holes in a board; *cf.* the *Industrial Fatigue Research Board's Report,* No. 16, p. 10);

(*s*) *Finger Co-ordination* (depressing the keys of three Veeder mechanical counters in succession with different fingers; *e.g.* with thumb for the first, second finger for the second, and little finger for the third. The counters were so placed that no arm movement was involved in the performance of the test);

(*t*) a *choice visual reaction time test* (three coloured lights were used for stimuli. The apparatus used is described in this *Journal,* 1921, XII. 147).

Under (8):

(*u*) *Spelling tests:* (*α*) that given by Link in *Employment Psychology* (Appendix); (*β*) a selection from Burt's material for a spelling test (see Ballard's *Mental Tests,* p. 158);

(*v*) a *Vocabulary test* (the test used was that given by Trabue and Stockbridge in *Measure your Mind,* p. 211).

(*c*) *Method of the Investigations.* The general procedure followed in these investigations was that briefly described in the opening paragraph of this report. An attempt was made to obtain from those who, as supervisors or teachers, were directly acquainted with any group of typists, a grading of the members of the group for typewriting efficiency; and when this was obtained, it was compared by means of the method of correlation (described and illustrated in the *Industrial Fatigue Research Board's Report,* No. 16, pp. 20, 51) with the grading of the group for efficiency in such tests as were given to it. The results in correlation are given in the next section. For obvious reasons it was impossible to give *all* the tests just described (Section III, *b*) to each group: for instance, they would have taken up too much time. As it was, the testing of each subject never occupied less than half-an-hour, and it was generally longer than this—in some groups a full hour. All the groups except C_1 and C_2 were tested individually: the members of these groups were tested all together.

As much care as possible was taken to get each group of subjects graded reliably for typewriting efficiency. Some of the difficulties experienced here may be judged from the fact that the efficiency grading given by one overseer corresponded exactly with the relative lengths of the periods during which the members of his group of typists had been in the employment of his firm: that is, he placed the one who had been longest with the firm first for typewriting efficiency, the one who had been next longest with the firm second for typewriting efficiency, and so on. When it was pointed out to him that such a coincidence seemed improbable, he made a few alterations, though not very many. Consequently little confidence was felt in this particular grading (this was the E group). Again, in the case of another group (F) the grading was greatly influenced by the class of work on which the different 'typists' were engaged, and was not determined simply by typewriting efficiency. Each of the members of this group had various duties of one kind or another in addition to typewriting, and some of these duties involved much more responsibility than others. It was impossible to obtain any grading from the overseer which was not influenced by these varying degrees of responsibility.

The question might very well arise here: why not give an actual typewriting test, and base the grading of a group upon that? This in fact was done with certain groups; but it was generally maintained by overseers that their own knowledge of a typist's usual work would be much more valuable than the result of such a test. In any case, it is not possible to compare several typists, with a view to discovering their relative capacities for typewriting by means of a short typewriting test, unless we can assume that they have had about the same amount of typewriting experience. And this cannot be assumed of the members of any groups of typists found in any business office. An experienced supervisor, however, can possibly allow for such differences to some extent, provided he is aware of them, as he sometimes is; but such allowances must be always more or less unreliable, and are quite unsatisfactory for scientific purposes.

The investigations were undertaken with a fairly clear recognition of the difficulties likely to be encountered in obtaining gradings of groups of typists for typewriting efficiency which could be accepted as indicative of relative typewriting *capacity*. All that need be said here is that every effort was made to make the grading of each group as reliable as it could be made.

IV. RESULTS.

The correlation coefficients of typewriting efficiency and the various tests will first be given for each group separately; after which the results for the different groups will be brought together so as to shew to what extent the coefficients of the *same* tests (with typewriting efficiency) in different groups of subjects were in agreement. The formula used throughout in obtaining the correlation coefficients is

$$r = 1 - \frac{6S(d^2)}{n(n^2 - 1)}.$$

No other tests were used with any group but those for which coefficients are given. The letters in front of the names of the tests are the reference letters given to the tests in § III (*b*). The 'probable errors' of the coefficients were not in general calculated, as they were not required for the arguments.

1. The A Groups (individual tests, occupying about one hour for each subject).

Group A_1 (24 young women typists).

Tests	Correlation coefficients	Tests	Correlation coefficients
(*a*, *α*) Cancellation ...	·13	(*f*) Immediate Memory Span (digits)	·05
(*i*, *α*) Opposites ...	·33	(*j*) Completion	·45
(*g*) Verb-object ...	·43	(*d*, *γ*) Substitution	·00
(*h*) Action-agent ...	·27	(*s*) Finger co-ordination	·00

One of the typists in this group was near the top in the tests, and almost at the bottom in the grading for typewriting efficiency. When it was pointed out to the supervisor that this subject had obtained a high place in the tests, the reply was that she *could* do very good work, *if she chose*, but she was very 'difficult' and was known in the office as a 'growser.' It was thus clear that for typewriting *capacity* she should *not* have been graded near the bottom of the group, a position which represented *her value to the firm*, and was determined by 'temperamental' qualities.

The typewriting *speed* (corrected for inaccuracy) of 17 of these subjects was known, as they had taken a speed examination. The coefficients of their ranking in this examination with their rankings in the above eight tests were in every case non-significant. The coefficient for the co-ordination test and typewriting speed was ·25.

Group A_2. (This group contained 20 young women typists; but six of them had been tested before as members of Group A_1. Coefficients were calculated from the whole 20 subjects as a group, and also for the 14 new subjects as a group. These correlation groups will be referred to as A_{2a} and A_{2b}.)

Tests	Correlation coefficients	
	Group of 20 (A_{2a})	Group of 14 (A_{2b})
(*e*) Immediate Memory Span (sentences)	·52	·64
(*d*, *α*) Substitution	·20	·10
(*b*) Finding test	·42	·62
(*p*) Touch cards	·10	·46
(*q*) Equivalence in grip	·41	·22
(*k*) Directions test	·27	·33
(*m*) Analysing table	·16	·27
(*u*, *α*) Spelling	·45	·81

Group A_3 (18 young women typists).

	Tests	Correlation coefficients		Tests	Correlation coefficients
(e)	Immediate Memory Span (sentences)	·59	(q)	Equivalence in grip	·30
(d, a)	Substitution	·00	(k)	Directions test ...	·41
(b)	Finding test	·50	(m)	Analysing table ...	·68
(p)	Touch cards	·44	(u, a)	Spelling	·58

2. The B Group (22 young women typists—individual tests).

The grading of this group for typewriting efficiency was based upon an examination in typewriting from copy and typewriting from dictation (errors, arrangement, and speed being taken into account). Correlation coefficients were calculated between the psychological tests and the results of this complete typewriting examination, and also between the tests and the results for the typing from copy part of the examination alone. Further, these subjects took a 'general knowledge' examination about the time the psychological tests were given them, and the results of this examination and the tests were also calculated. Finally, the group was graded on the results of about three months' industrial work at typewriting, this grading being based upon the supervisor's experience of the regular work of the subjects and a short weekly typewriting examination (from copy).

		Correlation coefficients			
	Tests	Typing from copy (1)	Typing from copy and dictation (2)	Three months' work (3)	General knowledge (4)
(e)	Immediate Memory Span (sentences)	·72	·68	·22	·62
(b)	Finding test	·47	·40	·49	·51
(j)	Completion test	·43	·32	·31	·55
(g)	Verb-object test ...	·23	·03	·00	·05
(d, a)	Substitution test ...	·03	·00	·17	·15

The coefficient of the complete typewriting examination (2) and general knowledge (4) combined, with the three months' industrial work was ·56.

The coefficient of experience at typing (length of time engaged on typing) with the three first tests alone (*e*, *b*, and *j*) was − ·32.

3. The C Groups (the subjects in Groups C_1 and C_2 were tested all together, those in C_3 individually).

For the purpose of grading the C groups, a curve was first drawn representing the experience of the school in improvement in typewriting: that is, different typing speeds, corrected for quality of work, were plotted against the numbers of hours practice *normally* required to attain them. This curve was then used as a standard, the grading of any student being determined as follows. The number of hours' practice *normally* taken to reach *his* speed was divided by the number of hours' practice *he* had taken to reach it. If then, he were of average typewriting capacity (that is, if he had reached his speed after just so many hours' practice as were on the average taken to reach that speed) the quotient of this division would = 1. If he were above the average typewriting capacity this quotient would be greater than 1, and if below average typewriting capacity it would be less than 1.

The members of the groups were then arranged according to the size of these quotients, the students with the largest quotients being placed at the top of their group, those with the smallest quotients at the bottom, and so on. Care was taken to exclude from the groups all students with regard to whom it was doubtful how many hours' practice at typewriting they had had; *e.g.*, all those who had ever used a typewriter outside school hours.

The reliability of these gradings depends, partly, upon the reliability of the standard curve. But, of course, one has to suppose that the members of the groups were all trying to learn with equal eagerness.

Groups C_1 *and* C_2 (60 and 22 subjects respectively. Mostly young women. Ages of members of larger group 15 to 21, of smaller 22 to 33).

Tests	Correlation coefficients	
	Group C_1	Group C_2
(*a*, γ) Cancellation	·34	·06
(*a*, β) Cancellation	·18	·20
(*c*) Sensory Discrimination	·12	·40
(*d*, *a*) Substitution	·02	·54
(*d*, β) Substitution	·28	·42
(*i*, *a*) Opposites	·24	·18
(*l*) Discrimination of Meanings	·32	·10
(*m*) Analysing table	− ·17	·34
(*j*) Completion	·08	·42
(*k*) Directions	·26	·72
(*u*, *a*) Spelling	·15	·28
(*v*) Vocabulary	·28	·24

It will be seen that there is some tendency for the coefficients to be higher with the older group than with the younger.

Group C_3 (30 subjects, young women under 21. Tests given individually. All these subjects had been members of Group C_1).

Tests	Correlation coefficients	Tests	Correlation coefficients
(*a*, γ) Cancellation	− ·23	(*q*) Equivalence of two hands in grip	− ·50
(*b*) Finding test	·24	(*r*) Match stick insertion test	·22
(*i*, β) Opposites	− ·14	(*t*) Choice visual reaction-time	− ·08
(*e*) Immediate Memory Span (sentences)	·04	(*u*, β) Spelling	·16

4. The D Groups ($D_1 = 12$ subjects, $D_2 = 14$ subjects. The D_1 subjects had been learning typewriting about 7 months, the D_2 subjects about 11 months. The grading of these subjects in typewriting was made on the basis of a school examination in typing from copy).

Tests	Correlation coefficients	
	Group D_1	Group D_2
Army Alpha No. 1	·15	·10
,, ,, No. 2	·21	·45
,, ,, No. 3	·27	·77
,, ,, No. 4	·00	·00
,, ,, No. 5	·46	·41
,, ,, No. 6	−·40	·69
,, ,, No. 7	·00	·60
,, ,, No. 8	·29	·00
Whole ,, Test	·20	·26
(*b*) Finding test	·33	−·40
(*d*, *a*) Substitution	·24	−·20
(*e*) Immediate Memory Span (sentences)	·38	·18
(*f*) Immediate Memory Span (digits)	·07	·16
(*m*) Analysing table	·40	·15
(*k*) Directions	·29	·13
(*j*) Completion	·48	·08
(*u*, *a*) Spelling	·68	·13

These subjects all belonged to the same district in London; they had had the same teacher for typewriting; yet the differences in the coefficients for the same tests with the two groups are often very considerable. In both groups there were 'erratic' individuals, and if these are omitted (one from D_1 and two from D_2) many of the coefficients are appreciably increased in size; but the differences between the two groups remain. There seemed no special reason for their occurrence.

5. The E Group (10 young women typists).

The office in which these typists were employed was a particularly busy one, and the tests were given in an atmosphere of rush. The subjects appeared to be in some dread of the results, though it was explained to them that they would not be affected by taking the tests. The grading was based on the supervisor's knowledge of the work of the subjects, none of whom had been under him for fewer than 12 months; but its unsatisfactory nature may be inferred from the fact that he at first declared that typewriting efficiency was *purely* a question of experience in typewriting. This opinion he afterwards modified, but its influence still had a noticeable effect in his grading of the group (*cf. supra*, § III, *c*).

	Tests	Correlation coefficients		Tests	Correlation coefficients
(*a*, *a*)	Cancellation	·24	(*f*)	Immediate Memory Span (digits)	·06
(*d*, *γ*)	Substitution	·59	(*i*, *a*)	Opposites	·00
(*h*)	Action-agent	·00	(*j*)	Completion	·26

6. The F Group (10 young women typists).

The members of this group were given the Stanford-Binet intelligence test. The manager of the office where they were engaged could not, however, grade the group for typewriting efficiency alone. All he could do was to state the kind of work on which each was engaged, whether this work was done satisfactorily or not, and what his *opinion* was as to the capacities of the subjects generally. This came to little more than an 'intelligence' grading, as the subjects were all engaged on different sorts of work. The intelligence quotients given by the test are therefore presented individually with such remarks as seem relevant in each case. The results are in accord with the common view that this test does distinguish between different degrees of general intelligence.

Subjects	Stanford-Binet I.Q.
A (assistant-secretary and head of clerical staff) ...	114·6
B (shorthand, typing, book-keeping—'satisfactory')	112
C (secretarial work—'very satisfactory')	111
D (typing mostly—a little shorthand)	100
E (typing only—accuracy required)	100
F (invoice clerk)	100
G (mostly typing)	99·5
H (shorthand and typing)	97
I (typist simply—routine work)	97
J (telephone operator—very little typing)	94

Table Z.

Shewing coefficients of correlation between typewriting efficiency and the same psychological tests with different groups of subjects.

Tests	Correlation coefficients for the various groups with gradings for typewriters												
	A_1	A_{2a}	A_{2b}	A_3	B	C_1	C_2	C_3	D_1	D_2	E	Average for all groups	M.V
(e) Immediate Memory (sentences)	—	·52	·64	·59	·72	—	—	·04	·33	·18	—	·43	·184
(u) Spelling	—	·45	·81	·58	—	·15	·28	·16	·38	·18	—	·40	·225
(k) Directions	—	·27	·33	·41	—	·26	·72	—	·29	·13	—	·34	·124
(p) Touch cards	—	·10	·46	·44	—	—	—	—	—	—	—	·33	·157
(g) Verb-object	·43	—	—	—	·23	—	—	—	—	—	—	·33	·100
(b) Finding	—	·42	·62	·50	·47	—	—	·24	·33	−·40	—	·31	·224
(j) Completion	·45	—	—	—	·43	·08	·42	—	·48	·08	·23	·31	·150
(m) Analysing table	—	·16	·27	·68	—	−·17	·34	—	·40	·15	—	·26	·184
(c) Discrimination (sensory) ...	—	—	—	—	—	·12	·40	—	—	—	—	·26	·140
(i) Opposites	·33	—	—	—	—	·24	·18	·14	—	—	·00	·18	·086
(d) Substitution	·00	·20	·10	·00	·03	·15	·48	—	·24	−·20	·59	·16	·175
(h) Action-agent	·27	—	—	—	—	—	—	—	—	—	·00	·13	·135
(a) Cancellation	·13	—	—	—	—	·26	·13	−·23	—	—	·24	·11	·130
(q) Equivalence in grip	—	·41	·22	·30	—	—	—	−·50	—	—	—	·11	·302
(l) Discrimination of meanings ...	—	—	—	—	—	·32	·10	—	·00*	·00*	—	·10	·105
(f) Immediate Memory (digits) ...	·05	—	—	—	—	—	—	—	·07	·16	·06	·08	·035

* Result for Army Alpha No. 4, which is identical in principle with the 'Discrimination of Meanings' test given to Groups C_1 and C_2.

In order now to bring out certain important points, the major part of the foregoing results are placed together in the above table (Z). For each test (except the Army Alpha tests) that was given to at least two groups, the coefficients obtained with all the different groups to which it was given, are there presented, along with the *average* coefficient for each test, and the mean *variation*. A dash in the table means that the test indicated was not given to the group indicated. The coefficients for all the *Army Alpha* tests with Groups D_1 and D_3 (given above) should also be compared in considering this table.

V. DISCUSSION.

(1) The most important feature of the results is the variation in the size of the correlation coefficients obtained from different groups with identical tests—identical not only in principle, but often also in material. This simply upsets the usual procedure in vocational investigations; that is, the drawing of conclusions as to the vocational value of particular tests for specified occupations from coefficients obtained from a single group of industrial subjects. For there can be no doubt whatever that the size of a coefficient of a test with occupational efficiency is partly a function of the group to which the test is given.

This result is in agreement with many earlier experimental results. For instance, Burt's data in "Experimental Tests of General Intelligence" (this *Journal*, 1909–10, III. 177) shew that intercorrelations of the same tests may be *very* different with two different groups. Burt gives the following figures among many others shewing similar features (the figures are 'corrected coefficients'):

Tests correlated together	Subjects from an elementary school	Subjects from a preparatory school
Card dealing and Tapping	·79	·36
Sound	·41	−·06
Lines	·59	−·07
Touch	·32	−·51
Weight	·05	−·40

Such results, which could be supported by results obtained by other investigators, render any coefficient derived from a single relatively small group of subjects practically useless.

The *range* exhibited among coefficients obtained from different groups with the same tests is in the present results often large. Thus (*cf.* Table Z) *immediate memory span* (sentences) yields a coefficient of ·72 with one group, and a coefficient of ·04 with another; *spelling* yields a coefficient of ·81 with one group, and a coefficient of ·13 with another;

directions yields a coefficient of ·72 with one group, and a coefficient of ·13 with another; *finding* yields a coefficient of ·62 with one group, and a coefficient of – ·40 with another; *equivalence in grip of the two hands* yields a coefficient of ·41 with one group, and a coefficient of – ·50 with another; and so on. It is therefore certain that obtaining a definitely significant (large positive) coefficient with one group of subjects affords little reason to suppose that similar coefficients will be obtained with other groups. This agrees with the inconsistency of the results obtained by earlier investigators, referred to above (§ II); and notably with the inconsistency of the results obtained by one particular investigator from two groups (see § I, p. 346).

The practical consequence of this result is clearly important, as may be realised from a hypothetical case. Suppose some test has been given to a particular industrial group and has yielded a coefficient of ·72 with this group's occupational efficiency. Suppose also a new group of workers is to be engaged and that this test is used by the employment office for selection purposes. It is evidently possible for the new group to yield a coefficient of ·04 between the test and its occupational efficiency. Let us suppose this to be the case. Since the test is now made the basis of selection for employment, those who do well in it are engaged and those who do badly in it are not engaged; hence, about as many persons are engaged who are *unfit* for the work as are *fitted* for it, and as many turned away who are *fitted* for it as are *unfitted* for it. If the coefficient of ·72 were an *average* obtained from a number of groups, the chance that this would happen would be considerably smaller than in the hypothetical case in which it is obtained from a single group; though even then it is a possibility to be reckoned with. On the other hand, it is questionable if a selection procedure based upon coefficients obtained from a single group (unless very much larger than industry as a rule can offer for experimental purposes) can be of any value whatever. If this general conclusion is valid, then all vocational test experiments so far made are practically useless.

The difficulties may be briefly stated thus. (*a*) We have to discover a test that will yield a high positive correlation, *which shall be valid*, with typewriting efficiency. This involves testing a large number of subjects, which means, practically, a considerable number of industrial groups. If we could find in this way a test which yielded a high positive *average* coefficient with typewriting efficiency, our investigation would so far have been successful; but the present investigations suggest that such success is difficult to attain. (*b*) Supposing, however, that success in this respect were attained, we should still, unless the average coefficient were *very* high, have to meet our old difficulty in a new place. It would be commercially valuable for a *large* firm to use the test; that is, if a large number of persons, either at one time or during a number of years, were selected on the basis of performance in it. It would also be of value to industry as a whole if employed generally. With a large number of persons such a test would guarantee a *general* adaptation of the worker to the work. With a small number, however, this result might not be attained, and in a given case it is *possible* that the use of the test would be commercially and individually valueless.

Investigators appear generally to believe that the ratio of a correlation coefficient to its probable error may be taken to indicate the practical validity or invalidity of the coefficient. But the fact is that, in the ordinary sort of investigation, this ratio is practically useless; for if the coefficient is small, the test that yields it is of little use, even if the coefficient is significant; and a coefficient (for a given test) may be ten times its probable error with one group, and nevertheless be approximately zero with another group. Thus, the coefficient for *immediate memory span* (sentences) in the present investigations was ·72 (p.e. = ·072) with the B Group of subjects, and ·04 with the C_3 group.

(2) The question naturally arises why it was that the coefficients of a given test and typewriting efficiency (in the investigations described in this report) varied so much with different groups of subjects. It is here suggested that the chief cause of this variation was that the efficiency gradings for typewriting were not determined by typewriting *capacity* alone, the extent to which other factors influenced these gradings differing in different groups of subjects. Some of the efficiency gradings, it is suggested, were closer approximations to *true capacity* gradings than were others, though we cannot be sure which these were, and it seems impossible that in similar investigations the matter should ever stand otherwise. A supervisor cannot help being influenced very largely by his knowledge of any given workmen's relative *commercial value*; and it cannot be supposed that this is a true indication of relative *capacity* for an occupation, although relative capacity will probably in most cases have some effect upon it. It is not necessary to enter into detail concerning the influence of such factors as experience of the work, methods of learning the work, and personal, social and domestic relations, upon occupational efficiency; as these are obvious enough. And a grading is not much more indicative of relative *capacity* when based upon the results of an occupational test, such as, in the present

investigations, an examination in typewriting, than upon a supervisor's judgment. For the results of such an examination are dependent upon amount of occupational experience, method of learning, and so on. It is simply not true that the typists who would be found in any business office, and who might constitute a group for experimental purposes, are equal either as regards amount of typewriting experience or method of learning to typewrite (not to mention social factors).

A probable subsidiary cause of the variation in the size of the coefficients should, however, be noted. The work included in the occupation of 'typist' shews considerable variety. In fact 'typists' may be classified under at least four distinct headings, according to the nature of their work. These are:

(*a*) secretarial or 'corresponding' shorthand typist;
(*b*) shorthand typist;
(*c*) typist (simply);
(*d*) typist and book-keeper.

Of these, the persons usually called *typists* are mostly 'shorthand typists,' though there is a large number of mere 'typists' (who spend their time copying invoices, etc.) while 'typing and book-keeping' is uncommon. Now different capacities are required for the above four classes of work; as is suggested by the fact that a given so-called typist may be able to do the work of one of the above four classes and not that of another (though this will to some extent depend on training, and not simply on capacity).

Thus, 'typists' of class (*a*) seem to require more 'general intelligence' than 'typists' of the other three classes (with which view the results for the F group agree) though they require also a better general education as well. Again, a mere typist, class (*c*) does not require either the 'intelligence' of class (*a*) or the capacities, whatever these may be, required for shorthand, which must, however, be possessed by classes (*a*) and (*b*). The educational director of the school where the C groups were tested insisted that the capacities for shorthand and for operating a typewriter are quite independent of each other. It is some confirmation of this view, that with groups C_1 and C_2 (the members of which were learning shorthand as well as typewriting) the correlation coefficients of shorthand with typewriting were $-\cdot 14$ and $\cdot 19$ respectively. Theoretical analysis of the two activities also agrees with this view; for, to mention but one point, in typewriting spelling is an important factor, whereas in shorthand spelling must be entirely neglected.

In so far as the work upon which the different groups were engaged was different, a grading for 'typewriting efficiency' of one group could not indicate the same capacities as a similar grading of another group. The fact would then be that, in the different groups, the tests were correlated with different, or partly different, *occupations*; in which circumstance, the coefficients might be expected to differ. But this factor certainly did not account for all the diversity in the results; for groups B, C_1 and C_2, and D_1 and D_2, were all graded on the very same sort of work—*typewriting simply*. Nevertheless, the coefficients obtained with any test from these groups exhibit considerable variation. The D Groups of themselves give rise to special difficulty, since here the subjects of the groups were very similar, and the method of teaching had been identical for the two groups; also, within each group, there had been exactly the same amount of typewriting practice, while the difference between the groups in this respect was not great (a few months). We here seem driven to refer to personal and social factors as the causes of the variation in the coefficients obtained with these groups (*e.g.* there might have been more desire to learn to typewrite in one of these groups than in the other—due perhaps to differences in the economic situations of the families concerned).

It is perhaps worth while emphasizing that the complexity of the occupation of 'typist,' and the consequent specialisation within the occupation (as indicated by the above classification) make it difficult to obtain here industrial groups suitable for vocational investigations of the usual kind; for business offices rarely contain a sufficient number of 'typists' engaged on any one sort of work (groups of typists 'simply,' class (*c*), could doubtless be found). That is to say, it is very difficult to obtain any industrial group of 'typists' who can be graded for *one occupation* (not to mention other difficulties referred to earlier).

One other factor which tends to influence the size of correlations may be mentioned. The more heterogeneous is a group, the higher will the intercorrelations of different performances of the group tend to be: different tests given to the group will tend to yield higher intercorrelations than the same tests given to more homogeneous groups. A good illustration of this fact will be found in B. R. Simpson's *Correlation of Mental Abilities* (Teachers College, Columbia University, Contributions to Education, No. 53, 1912; for details cf. the author's report on 'Motor Capacity,' § IV, 1). (*Cf.* also the *Industrial Fatigue Research Board's Report*, No. 12, p. 21, 2nd paragraph from the bottom.) But this knowledge cannot extricate us from the present difficulty. For it is hardly feasible to attempt to standardise the heterogeneity required in a group if 'valid' coefficients are to be obtained from it.

(3) In view of the difficulties that beset the correlation method in vocational guidance investigations, the question arises whether any other method might be adopted. The answer here tentatively suggested is that valuable results might be obtained by a greater emphasis on theoretical occupational analysis than has so far been made. Such analysis, starting from observation of an occupation, would aim at the enumeration of the psycho-physiological functions exercised in it. It would be relatively easy to determine the presence or absence of defined capacities in particular persons. As to the reliance that could be placed upon the results of such analyses, much would depend upon the scientist who performed them. There could never be direct demonstration of their validity; and it would appear to be a step forward if it is explicitly recognised that this is so. The justification of any such analysis could only come with time, and would take the form that persons who were engaged for an occupation by reference to this analysis, had better general health and shewed greater efficiency over a long period, than persons not thus selected. A skilled scientist should make no greater mistakes in such work than a doctor in his diagnoses.

The training of the scientist who would make such analyses is a question of the first importance. It would include a wide and thorough acquaintance with psychological tests. As the work of such a scientist is here conceived, the analyses would be considered in the light of all possible knowledge concerning the occupations to which they refer. Also, investigations similar to those described in this report would frequently be carried out; but their results would not be considered *to demonstrate* anything. Such investigations would be undertaken because it is known that in the course of making them important features of the occupations concerned would be brought to light. Correlation coefficients between particular tests and occupational efficiency would not be given great weight, except in very special cases (*e.g.* if it were recognised that a particular group was especially suitable for experimental purposes). They would be used as sources of suggestion, and might be expected to give some moderate confirmation of the analyses.

It is in accord with theoretical analysis of actual typewriter operating that, in these investigations, the *immediate memory span* (sentences), the *spelling*, and the *direction* tests, yielded average positive coefficients. These three tests in particular were included among those used because theoretical analysis of the work seemed to shew that the capacities they tested were exercised in it.

The method of exhaustively examining a number of those who excel in any given occupation probably deserves attention (*cf.* the *Industrial Fatigue Research Board's Report*, No. 12, p. 7). It has never been properly tried in industry (if at all), and yet certainly seems worth while trying. It would of necessity be slow, and would involve sociological as well as psycho-physiological investigations (*cf.* also the above-mentioned *Report*, Part III, pp. 50–51).

(4) We may now briefly consider whether any conclusions can be drawn from a comparison of the results of earlier experiments (analysed in § II) with the results of the present investigations, or from the latter results alone, with regard either to tests or capacities relevant to typewriting.

First, granting that the results of correlation experiments are determined *partly* by relevant capacity, we may say that it has been definitely established that the *substitution* test is valueless as an indication of typewriting aptitude (*cf.* above, pp. 354–7, and Table Z, p. 358). In the present investigations this test was given to 10 groups, and the coefficients obtained were negligible in all except two cases, and in one of these (Group E) the grading for typewriting efficiency was very unsatisfactory.

Secondly, our results seem to have confirmed Lahy's conclusion concerning *immediate memory*. At any rate, the *immediate memory span* test (with sentences as material) gave in our investigations a higher *average* coefficient than any other test.

Thirdly, our results for the *touch cards* test give some support to Lahy's conclusion concerning the relevance to typewriting of 'tactile sensibility,' though his method of testing this function was probably very different from that used in the present investigations. But our *touch cards* test was given to three groups only, and consequently little reliance can be placed on our average result.

Fourthly, our results tend to contradict Lahy's as regards the relevance to typewriting of 'muscular symmetry' (tendency to equivalence in the grip of the two hands); though here our average coefficient is derived from coefficients for four groups only. Still, one of these (C_3), a carefully graded group, yielded a coefficient of $-\cdot 50$ with this test.

Fifthly, our results for the *completion* test tend to confirm Link's conclusion with regard to this test; and our result as regards the acquired ability *to spell* is very definitely in agreement with Link.

Sixthly, in these investigations neither a 'muscular sensibility' test (Lahy) nor a *colour naming* test (Rogers) was used; and as regards the *verb-object* and *action-agent* tests with which Rogers obtained significant results, these were given to only two groups, and consequently little reliance can be placed on the results. From these two groups, however, the results for the *verb-object* test tend to confirm Rogers's result; while those for the *action-agent* test tend to contradict his result. Again, our result for the *opposites* test tends to confirm the result obtained by Rogers for this test; and also our result tends to confirm the conclusion of Lahy concerning the irrelevance to typewriting of 'attention' (the *cancellation* tests).

Finally, we may say that the present investigations *suggest* that the following tests might be useful in defined circumstances for vocational guidance in connexion with typewriting: (*a*) *immediate memory span* (sentences), (*b*) *direction*, (*c*) *finding*, (*d*) *completion*, and (*e*) *spelling* (though this last test is a test of definitely acquired knowledge). (*Touch cards* and *verb-object* are not included here because, although yielding a slightly higher average coefficient than either *finding* or *completion*, they were given to only three and two groups respectively.) Theoretical analysis of the typewriting activity agrees so far with this conclusion. The importance of *spelling*, for instance, is due to the fact that a typist does not copy the spelling in operating the typewriter, but reads a certain number of words (in doing which, owing to the 'illegibility' of much handwriting, the activity involved in a *completion test* is frequently called for) and transcribes, by means of the typewriter keys, the particular spelling of these words *that has been acquired*, whatever this may be; a procedure which is, of course, modified in the case of unusual or unknown words, such as names. During this process, the activity involved in the *finding* test also seems to be important, though doubtless less so where a typist uses the touch method than where both sight and touch are employed. And theoretical analysis suggests further the relevance to the typewriting activity of 'speed of association,' such as is involved in the *colour naming* test, since this 'function' would seem to be highly important in the serial reproduction of letters in *spelling*. Theoretical analysis also suggests that some minimum standard of bi-manual dexterity should be possessed by a typist; but since the rate of striking the keys, even when tapping at a fast speed, is far from the 'physiological limit' of rate of movement, this standard would probably not be high, and might, as a rule, be assumed.

(5) It might be said that the fact that the results of the present investigations are supposed to be in general agreement with results of a theoretical analysis of the typewriting activity (assumed to be correct) tends to invalidate the criticisms made above of the correlation method

in vocational guidance investigations. In answer to a possible remark of this sort, two replies may be given.

First, what has been primarily urged is the impossibility of accepting coefficients obtained with any single group of subjects as valid: at any rate, unless the group is much larger than the groups ordinarily procurable. If we are justified in supposing that the coefficients obtained in the present investigations can form the basis of argument, there can be no doubt of the truth of this conclusion. Of course, if, for any reason, the data presented above appear questionable, the situation will be altered. And it may, indeed, be legitimately contended that, seeing that the coefficients given are all 'raw,' too much emphasis has been placed upon discrepancies among them; and that our main conclusion is not sound unless the tests used were reliable. It is admitted that the reliability of these tests was not determined, and consequently that a certain doubt may be justified concerning our conclusion. While this is so, it should be said that the tests used were at least as reliable as psychological tests generally are. And, with regard to earlier investigations in vocational guidance, it may be pointed out that results have usually been given for tests whose reliability was unknown.

Other reasons also may be brought forward to impugn particular parts of the data; as, *e.g.*, that the D groups were small, that the grading for typewriting capacity of the E group was very unsatisfactory, that the similar grading of the C groups is doubtful[1], and so on. In so far as such reasons are sound, our main conclusion may be doubted; and it is not absolutely certain that they are not sound. But, then, it becomes essential to define the nature of the group, and the nature of the grading, which can be accepted as making the correlation procedure possible. How large must the group be? How 'heterogeneous' as regards occupational capacity? Precisely how is it to be graded for occupational capacity?—When these questions have been answered, the group possessing the required characteristics must be found. Assume that it is found; and that vocational tests are discovered. The various groups to which they will be applied, will certainly not all possess the qualities of the ideal group; and in so far as they do not, the tests may fail to grade them according to typewriting capacity.

Secondly, though average coefficients may doubtless be suggestive, they cannot reasonably be accepted as absolutely reliable (unless their $m.v.$ is negligible), even though in particular cases they *may* give a correct indication of the required capacities. Of course, average coefficients are better than coefficients obtained from single groups—which (as a rule) are not reliable at all. An average coefficient cannot be supposed to indicate correctly how far the capacity tested by the test that yields it is really relevant to the occupation concerned; the reason being simply that the coefficients from which it is derived will be more or less determined by factors other than relevant capacity. What an *average* coefficient, derived from a number of industrial groups, will permit of is this: a fairly reliable judgment as to how accurately we may predict occupational *efficiency* (not occupational capacity) from performance in given tests.

---

[1] Such points have been raised by Mr Cyril Burt, to whom I am indebted for valuable comments on my argument.

VI. CONCLUSIONS.

(1) In the absence of a standard as to what constitutes a satisfactory group for correlation investigations, no reliance can be placed upon a correlation coefficient between efficiency in a given test and efficiency in a particular occupation, when this coefficient has been obtained from only one industrial group (of 'ordinary' size); and, in particular, it cannot be inferred that the capacity tested by the test yielding the coefficient in question, is or is not essential to the occupation. Such an inference does not seem to be made more justifiable by pointing out (supposing it to be true) that the coefficient is four, five, or more times its probable error. For all that this would shew is that the coefficient was not due to chance; but its being due to chance is not the only alternative to its being due to the fact that the capacity tested is essential to the occupation.

(2) It is suggested that useful results might be obtained for vocational purposes by giving more attention to the theoretical analysis of occupations (such analysis being carried out with the purpose of arriving at the elementary functions exercised in the occupations). In making such analyses, all relevant knowledge would be utilised; and correlation experiments would be made, as likely to yield useful suggestions, though very much importance would not be attributed to coefficients thus obtained. But there is no direct method by which the results of a theoretical analysis of an occupation can be shewn to be valid. Demonstration of its validity would take the form that, over a long period of time, groups of workers selected on the basis of such analysis were more efficient and enjoyed better general health than unselected groups (in the same occupation).

(3) 'Typists' are divisible into several classes according to the

nature of their work (though all divisions of work involve the operating of a typewriter more or less). This is an important fact to bear in mind in any further vocational investigations into this occupation. The distinguishing mark of the highest class of typists ('assistant secretaries') seems to be, in addition to a general education, a relatively high degree of 'general intelligence.'

(4) The results of a theoretical analysis of the operating of a typewriter, and the results of the present correlation investigations, are in general agreement in suggesting that the following five tests might be useful for vocational guidance in typewriting: (*a*) *immediate memory span* (sentences), (*b*) *directions*, (*c*) *finding*, (*d*) *completion*, (*e*) *spelling* (this last test being a test of acquired ability). It is not unlikely also that a test of 'speed of association' would also be valuable, as this is suggested by theoretical analysis and is in agreement with the results obtained by Rogers. For typists of the 'assistant secretary' class, special emphasis would be laid upon 'intelligence' tests.

It may be pointed out finally that if, for any reasons, the results for the C, D, and E groups are less reliable than those for the A and B groups, the positive suggestions arising out of these investigations will increase in importance.

(*Manuscript received* 31 *May* 1922.)

THE TRANSFER OF IMPROVEMENT IN REASONING IN SCHOOL-CHILDREN

BY W. H. WINCH.

I. *General plan of the experiment* (p. 371).

II. *Preliminary tests* (pp. 372–376).

  (i) *The actual tests.*

  (ii) *Administration of the tests.*

  (iii) *Marking of the tests.*

  (iv) *Results of preliminary tests.*

III. *The practice period of the experiment* (pp. 376–379)

IV. *The final tests and their results* (pp. 379–381).

V. *Summary* (p. 381).

IN preceding papers[1], I have indicated, here and there, that a pedagogical factor, though, perhaps, a small one, may have been involved in the results of reasoning tests given to school-children. Certainly schools of varying curriculum and pedagogical attitudes—for the English school system is an elastic one—have shown, provided that the social class of the children attending the school remained the same, very uniform results, age for age, though there seems a sex-difference throughout in favour of the boys. Very few of the schools taught Functional Grammar seriously; but in most of them Problematic Arithmetic was treated more or less satisfactorily. These are the school subjects which, teachers almost unanimously assert, are akin to Logical Reasoning, and involve so closely the same or similar mental functions that improvement in the one causes improvement in the other. In the phraseology of experimental pedagogy they would expect 'transfer' in these cases. The correlation between Logical Reasoning and Arithmetical Problematic Reasoning is fairly high. Is that conclusive as to community of function? I fear not; correlations are often high where no 'transfer' effect can be found. How then can we solve the problem? In two ways:

---

[1] "Some New Reasoning Tests suitable for the Mental Examination of School-Children." This *Journal*, 1915. "Children's Reasonings." *J. of Exp. Ped.* 1921, 1922.

the analytical psychologist can show, or attempt to show, that the same principles of thought are involved. (In the present state of knowledge I set aside all inferences from hypotheses about nerve-centres and nerve-fibres as quite inadequate—even if they ever will be otherwise.) Or, we can, exercising due precautions, actually test whether improvement in the one set of functions does or does not produce improvement in the other, and this is the problem to which the following paper is devoted. Does improvement in Arithmetical Problematic Reasoning involve an improvement in Logical Reasoning which is not arithmetical? Are we, in teaching problems in arithmetic, not only helping the pupil to perform the arithmetical operations which will confront him in his daily life, but also helping to make him a better reasoner in other ways? The vast majority of teachers have no doubt that we are; much current psychology would assert that we are not. It is not an easy question to decide. For example, in England, at any rate, we think that all arithmetical functions are rather highly correlated: yet I have found that a great improvement in arithmetical computation may leave arithmetical reasoning unaltered.

I. GENERAL PLAN OF THE EXPERIMENT.

The experiment was undertaken with Standards VI *b* and V, consisting of 58 girls of an average age of 12 years 3 months on March 31st, 1913. Although this class consisted of two grades or standards, they were taught together under one teacher: the work commenced on Thursday, February 13th, 1913, and ended on Tuesday, July 8th, 1913.

The first step in the procedure was to obtain an adequate measure of each girl's capacity in logical and arithmetical reasoning. Six sets of logical tests and six sets of arithmetical problems were given (the questions are given below) at proper intervals. I say at proper intervals, for to crowd these exercises one on another without proper intervals will only produce fatigue or boredom; which, indeed, can be easily seen by the irregularity of the results produced under such conditions. Then the girls were divided into *two equal, parallel and highly-correlated groups* on the results of these logical exercises. One of these groups was subsequently trained specially in problematic arithmetic; the other was not. Finally, both groups worked the logical and arithmetical exercises again. Which group did the better, and how much, if any better?

I need hardly say that all the work was *unwissentlich*: none of the girls knew the purpose of the experiment.

II. PRELIMINARY TESTS.

(i) *The Actual Tests.*

The following are the six sets of Logical Tests. All of them are new and were expressly devised by me.

SET A.

1. Johnny went to see his aunt. There were only three ways in which he could go, for it was too far to walk. He could go by train or by 'bus or by tramcar. He did not go by tramcar or by train. Can you tell how he did go? If you can, say what makes you think so. If you cannot tell, say why you cannot tell.

2. Tommy said, "All the boys in our Sunday School except me have gone this morning for an excursion." Just after he said that, another boy who went to the same Sunday School was seen playing in the street. Was Tommy right in what he said? You must give a reason for your answer.

3. There were three boys, Tom, Dick and Harry, and they were friends. But they did not like going out all three together, they liked to go out two at a time. Write down the names of all the different pairs that they could make up so as to go out two together each time.

4. All the butchers' shops in a long road were painted red. If you were in that road and you saw a shop painted red, would you be certain it was a butcher's shop, or would you not? You must give a reason for your answer.

SET B.

1. Harry was as tall as John, and John was as tall as Tom. Was Harry taller than Tom, or was he shorter than Tom, or was he just as tall as Tom, or can't you tell? You must give a reason for your answer.

2. "If I get all my sums right this week Mother has promised me a penny" said little Mary. Mary always spoke the truth and Mary's mother always kept her promises. Mary did not get the penny. Do you think she got all her sums right or do you think she did not? You must say what makes you think so.

3. To get a prize in South Street School a girl must never stop away from school, and she must always come early, and lose no marks for bad conduct. Jane lost no marks for conduct and she was never late. Can you tell whether Jane got a prize or not? You must say what makes you think so.

4. Some of the boys who go to Edward Street School wear red caps. If you met a boy who went to that school would you expect he would wear a red cap, or would you expect he would not have a red cap on, or would you be uncertain? You must say what makes you think so.

SET C.

1. Alice is taller than Kate and Kate is taller than Mary. Is Alice taller than Mary, or just as tall as Mary or shorter than Mary or can't you tell? You must say what makes you think so.

2. Every boy who got a good mark in reading got a prize. Johnny got a good mark in reading. Did he get a prize or did he not? If you think he got a prize you must say why he got it. If you think he did not get a prize, you must say why he did not get it.

3. There is a large lake in a forest, and the forest is in a country called Firland. Which is the larger, the lake or the whole country of Firland? You must give a reason for your answer.

4. A table is on the right-hand side of a chair, and a blackboard is on the right-hand side of the table. Is the blackboard on the right-hand side of the chair or on the left-hand side of the chair? You must say what makes you give the answer you do.

SET D.

1. There were three flaps on the backs of three seats on the top of a tram-car, and the flaps were moving backwards and forwards. One of them was making a creaking noise as it moved. Suppose you could not find out which one it was by listening whilst they were all moving, what would you do to find out which one it was? You must give reasons for your answer.

2. John and James both walked from a town called *A* to a town called *B*. John walked first to a town called *C*, and then, without stopping, he walked from *C* to *B*. But James walked on a road from *A* to *B* which did not go through *C*. Can you tell who went the shorter way? If you can tell, say what makes you think so. If you can't tell, say why you can't tell.

3. Every girl who went to a party had either an apple or an orange given to her to take home. Mary went to the party. Did she get an apple or did she get an orange? When you have answered this question, say if you are certain that she got one of them? Give a reason for your second answer.

4. There were four clocks in a house which for a long while kept correct time. By-and-by three of them appeared to be faster than the other one, and all these three clocks seemed to gain the same amount each day as compared with the other one. Which do you think was more likely to be right, the three clocks which were faster, or the one which was slower? You must say why you think so.

SET E.

1. Mary sowed some seeds in some flower-pots, and every few days she watered them. Johnny said that he did not see that putting water on the seeds did any good. Do you think that Mary could prove to him that watering them made them come up quicker or do you not? If you think she could, say how you think she could do it. If you think she could not, say why not.

2. Do you think a shilling is larger than a halfpenny or smaller than a halfpenny or the same size? Suppose you had a halfpenny and a shilling and nothing else to measure with, how could you find out?

3 Harry and Tom live in a long road a long way away from each other. Harry

starts from his house to go to meet Tom, and Tom starts from his house to go to meet Harry. They both start at exactly the same time, but Tom walks faster than Harry. Do you think they will meet nearer Tom's house or nearer Harry's house, or exactly in the middle? You must give a reason for your answer.

4. If more than half the boys in a class get all their sums right, and more than half the boys in the same class get no mistakes in dictation, do you think there were any boys who got all their sums right and also no mistakes in dictation, or do you think there were no boys who got all their sums right and also no mistakes, or can't you tell? You must say why.

SET F.

1. Tom was heavier than Dick and Dick was lighter than Harry. Was Tom heavier than Harry, or was he lighter than Harry, or was he the same weight, or can't you tell? You must give a reason for your answer.

2. If the only places in a town where you can buy marbles are toy-shops, and all the toy-shops in that town are in Burton Road, would it be true to say that the only places in that town where you can buy marbles are in the Burton Road? You must say why you give the answer you do.

3. William's father, who always kept his promises, said to him, "If you get all your sums right for a week, and lose no marks for spelling during that week, I will give you a shilling." William did not get the shilling. Do you think he got all his sums right? Do you think he lost any marks for spelling? Do you think he lost marks for both sums and spelling? You must give a reason for your answer.

4. It was known that a theft had been committed one night from a private house, either by a soldier or a sailor, and there was no more reason to think it was a soldier than a sailor. Only one sailor was absent on leave from his ship that night, whilst ten soldiers were on leave away from their barracks. Each soldier went by himself that night. Do you think the theft was more likely to have been committed by a sailor or by one of the soldiers? You must give a reason for your answer.

I do not propose to set out in full the six sets of tests which were given in Problematic Arithmetic. They must, of course, be of such difficulty that all the pupils taking part in the experiment could do some, and none of them could do all successfully. One set, given merely for illustrative purposes, follows:

SET II.

1. A gardener made 3 circular flower beds and planted wall-flowers all round the edges. The first flower bed measured 80 feet round, the second and third each measured 60 feet round. If he placed the wall-flowers 6 inches apart, how many plants would he require?

2. Two men, Jones and Brown, set out together to walk to a town 160 miles

away. Jones walked 10 miles a day, whilst Brown walked 8 miles a day. How many days' journey would Brown be behind Jones, when Jones reached the town?

3. A dairyman bought 100 pounds of butter at one shilling a pound. He mixed it with 25 pounds of lard at sixpence a pound and one pound of colouring matter at tenpence a pound. What was his profit supposing he sold the mixture at one shilling and twopence a pound?

4. At a concert 150 two-shilling tickets were sold, 200 shilling tickets and 300 sixpenny tickets. The expenses of the concert were £8: what profit was there?

(ii) *Administration of the Tests.*

The tests were written on a blackboard, one problem at a time: they were read aloud twice, without undue emphasis, by the teacher, and the children were told to think carefully before answering them. The second problem was not given until each child had done all she could with the first. No time-limit was imposed. I well understand the value of short, sharp answering for certain purposes; but, for such a purpose as the present one, the 'answer in a moment or you are lost' attitude is most undesirable. The following is the chronology of the Preliminary Tests:

1st	test in	Logical Reasoning	—Thursday	11.15 a.m.	February	13th,	1913
1st	,,	Problematic Arithmetic	—Friday	,,	,,	14th	,,
2nd	,,	Logical Reasoning	—Monday	,,	,,	17th	,,
2nd	,,	Problematic Arithmetic	—Tuesday	,,	,,	18th	,,
3rd	,,	Logical Reasoning	—Wednesday	,,	,,	19th	,,
3rd	,,	Problematic Arithmetic	—Thursday	,,	,,	20th	,,
4th	,,	Logical Reasoning	—Friday	,,	,,	21st	,,
4th	,,	Problematic Arithmetic	—Monday	,,	,,	24th	,,
5th	,,	Logical Reasoning	—Tuesday	,,	,,	25th	,,
5th	,,	Problematic Arithmetic	—Wednesday	,,	,,	26th	,,
6th	,,	Logical Reasoning	—Thursday	,,	,,	27th	,,
6th	,,	Problematic Arithmetic	—Friday	,,	,,	28th	,,

(iii) *Marking of the Tests.*

The answers in Logic were marked merely 'right' or 'wrong,' an adequate reason being required before the solution was accepted as correct. Then they were re-marked by being weighted for difficulty: those answered correctly by all or nearly all received the lowest mark, whilst those worked correctly by few received the highest mark. Approximately, the mark was inversely as the frequency of accuracy.

The answers in Problematic Arithmetic were marked 'right' or 'wrong' if adequate steps in reasoning had been made which would enable the correct solution to be reached. Errors in computation were not considered. Then they were re-marked by being weighted for difficulty on the same plan as that adopted for the logical exercises.

I may say at once that a definite conclusion emerges on either method of marking; but, as one would expect, more clearly by the second system of marking.

(iv) *Results of Preliminary Tests.*

There were 58 cases in all, and the correlations between the series were worked out directly from the individual cases by the well-known Pearson 'r' formula.

The coefficients of correlation follow:

Between Logic and Arithmetic (one mark each correct answer) = ·482 (p.e. ·068)
" " (marks weighted for difficulty) = ·504 (p.e. ·066)
Between two markings of Logic = ·930 (p.e. ·012)
" " Arithmetic = ·933 (p.e. ·012)

It was thought that a division into *two equal, parallel and highly correlated groups* might be safely made and this was done on the basis of the marks obtained for the logical exercises. Then followed the second or practice period of the experiment.

III. THE PRACTICE PERIOD OF THE EXPERIMENT.

The next step, or practice period of the experiment, was employed in this way. One of the two equal groups received a number of lessons in Problematic Arithmetic. The problems set in the Preliminary Tests were made the basis of the lessons (which were 38 in number) and the children solved a number of questions embodying the principles involved. An illustrative lesson follows.

The question dealt with is number 2 of Set II of the problems previously cited. The teacher wrote the following preliminary questions on the blackboard.

1. Tom walks to Guildford: it is 16 miles from his home. If he walks at 4 miles an hour, how many hours will he take to get there?
2. Tom cycles back at the rate of 8 miles an hour, how long will it take him to get home?
3. A horse trots 21 miles at the rate of 7 miles an hour; how many hours does it take him?
4. A man walks the same distance at 3 miles an hour; how many hours does it take the man?

These questions being answered satisfactorily, the teacher then asked the girls how they had worked the sums. They told her:

1. "By division." "Division of what?" said the teacher. "Of one into the other." "Can anyone give a clearer answer?"

2. "Divide the distance by the number of miles." "What distance and what number of miles?"

3. "*Divide the distance the man or horse has to go by how many he can do in the hour, to see how long he takes.*" This answer was accepted and written on the blackboard. The teacher then called on a girl to use this rule to solve the first sum. The answer was, "It is 16 miles to Guildford, Tom goes 4 miles an hour, 4 into 16 goes 4: it takes him 4 hours." The teacher calling on another girl, said, "Nellie, show me how you can find out from this rule how long it takes him to come home." Nellie answered, "It is 16 miles to come home, he rides 8 miles in one hour and 8 more in 2 hours, that is the 16." "Who can put this more clearly?" said the teacher. One girl answered, "8 miles an hour: 16 miles to go: 8 into 16 goes 2, that is 2 hours."

The teacher asked another girl to apply the rule to question 3. The child said, "The horse trots 21 miles, he goes 7 miles an hour: 21 divided by 7 is 3: it takes him 3 hours at 7 miles an hour because 7 times 3 *is* 21."

The teacher resumed, "And the man in question 4, how long does it take him to go the 21 miles?" She was told, "The man goes 3 miles an hour: 3 into 21 = 7: he will take 7 hours."

The teacher then said, "Which would get there first, horse or man?" The answer was given, "Horse, in 3 hours." Then the question was asked, "How far would the man have gone when the horse was at the end of his journey?" The answer was "9 miles." "How did you get 9 miles?" "The horse takes 3 hours to get there, if the man walked 3 miles an hour, he would be 9 miles along the road." Then the following question was written on the blackboard.—

5. Tom rides to a town 24 miles away in 4 hours: John walks at the rate of 3 miles an hour: How far has John walked when Tom has got there?

The teacher asked for solutions. One girl said "Tom gets to the town in 4 hours, that gives John 4 hours," but she could not get any further. Another girl said, "John has got 4 hours to walk because Tom has finished in 4 hours; but John can only go 3 miles an hour, so he has only gone 12 miles." The teacher said, "What do you have to do to find out how far John has gone?" The answer was given, "*See how long it takes the quick one, and multiply the time the quick one takes by the number of miles an hour the slow one goes.*" This answer was accepted and written on the blackboard. The teacher proceeded, "How many miles has John got to go?" She was told "12 now." "How can you tell how much further the slow one has to go?" "*By subtracting the distance the slow one has gone from the whole distance,*" was the answer. As all the girls appeared to accept this statement, it was written on the blackboard.

The teacher went on, "How many miles has John yet to go?" She was told "12 miles." "How long will that take him?" The answer "4 hours" was given. "Supposing he could only go at 2 miles an hour, how long then?" "Six hours," was the answer. "If he walked at 4 miles an hour, how long then?" Answer "3 hours." "Now," said the teacher, "how did you find the number of hours each time?" The answer was given "*By dividing the distance he has yet to go by how much he is doing each hour.*" This was accepted and written on the blackboard.

The following examples were then set for the girls to work on paper:

1. Ben and Harry set out to walk 48 miles. Ben walks 8 miles a day, Harry 6 miles a day. Who would finish the journey first, and how much would the other be behind?

2. A horseman and a cyclist set out together to ride to a town 40 miles distant. The cyclist rides at 10 miles an hour, the horseman at 8 miles an hour. Which one arrives at the town first, and how many hours' journey is the other one behind?

3. Fanny and Alice have each 48 beads to thread, Fanny can thread 4 a minute, Alice 6 a minute. Which child threads all her beads first, and how many minutes' work has the other to do?

4. Two firms accept orders to make 150 motor-cars each. One turns them out at the rate of 5 a week; the other at the rate of 3 a week. How many more has the second to do when the first has completed the order?

It will be noticed that the actual problem given in the Preliminary Tests has *not* been solved, but the principles involving the solution have been taught and careful statements have been required as to procedure. Mere juggling with numbers until a correct solution appears, more or less by accident, is not problematic arithmetic and will have little if any 'transfer' value either to similar arithmetical problems or to any other subject.

The Group thus taught will be known henceforward as the 'Practised Group,' meaning by that, taught and practised in Arithmetical Problematic Reasoning. Meanwhile the other Group, now to be known as the 'Non-Practised Group,' took their ordinary time-table lessons under their own teacher—a strong and capable one—whilst the first group were having their special lessons. No other *problematic* arithmetic was taught to either group during the period of the experiment, though ordinary arithmetic lessons were given in 'rule sums' in accordance with the school time-table.

A diary of one week's lessons follows:

	Practised Group (Group A)	Non-Practised Group (Group B)
Tuesday, April 1st, 1913. 9.45 to 10.30	Problematic Arithmetic	Reading
Wednesday, April 2nd, 1913. 2 to 2.40	,, ,,	Dictation
Thursday, April 3rd, 1913. 9.45 to 10.30	,, ,,	Geography
Friday, April 4th, 1913. 9.45 to 10.25	,, ,,	Reading

There were, in all, ten weeks of special lessons, beginning on April 1st and ending on June 13th. It will be noted that a considerable interval elapsed after the end of the Preliminary Tests, occasioned partly by time devoted to marking the exercises and partly by school holidays. Nor was the Practice period quite continuous, even after it had begun, for the Whitsuntide holidays intervened and the Head Teacher, who taught the Problematic Arithmetic, was away for a time through illness. I do not regret this, for I wanted every suspicion of intensive work under pressure to be removed. Such work will often give rise to rapid improvement in a special function; but it is just the sort of work from which no 'transfer' effects can usually be expected. 'Cramming,' if the examination is a good one and not a mere repetition of a special syllabus, always defeats its own object. But of course an experiment spread over a long period has difficulties of its own. Some children leave school through exemption or removal; some will not 'pair'; some are absent through illness and other causes, and all these cases must be omitted in the final tabulation of results. Moreover, in the division into equal groups we must pay regard to age; for, if one group is much younger than another within the same class, even though they start equally, the younger group will forge ahead as time goes on. We felt, however, that we had adequately surmounted all these difficulties and on Monday, June 16th, we began to repeat all our Preliminary Tests with both groups as Final Tests.

IV. THE FINAL TESTS AND THEIR RESULTS.

The chronology of the final tests was as follows:

1st test in	Logical Reasoning	—Monday.	11.15 a.m.	June	16,	1913
1st	,,	Problematic Arithmetic	—Tuesday	,,	,,	17	,,
2nd	,,	Logical Reasoning	—Wednesday	,,	,,	18	,,
2nd	,,	Problematic Arithmetic	—Monday	,,	,,	23	,,
3rd	,,	Logical Reasoning	—Tuesday	,,	,,	24	,,
3rd	,,	Problematic Arithmetic	—Thursday	,,	,,	26	,,
4th	,,	Logical Reasoning	—Friday	,,	,,	27	,,
4th	,,	Problematic Arithmetic	—Monday	,,	,,	30	,,
5th	,,	Logical Reasoning	—Tuesday	,,	July	1	,,
5th	,,	Problematic Arithmetic	—Wednesday	,,	,,	2	,,
6th	,,	Logical Reasoning	—Thursday	,,	,,	3	,,
6th	,,	Problematic Arithmetic	—Tuesday	,,	,,	8	

The tests were administered exactly as before in the preliminary operations and were marked in two ways exactly as before. As I have already indicated, the numbers available for our final tabulation were much reduced from the 58 pupils who took the preliminary tests. The results follow:

Table I. *Showing the Results in Logical Reasoning of the two Groups compared.*

Marks weighted for difficulty

Practised Group (Initials only)	Preliminary Tests	Final Tests	Non-Practised Group (Initials only)	Preliminary Tests	Final Tests
C. W.	61	64	R. N	60	53
A. R.	49	71	K. K	49	43
B. B.	46	59	L. H.	46	51
I. E.	45	51	M. J	43	34
E. A.	42	53	F. T.	43	4[illegible]
I. B	42	50	A. M.	42	42
W. M.	41	43	E. W.	34	3[illegible]
H. C.	40	44	E. C.	40	36
A. P.	38	55	E. W.	38	3[illegible]
D. F.	36	56	M. H.	36	48
N. P.	34	51	J. W.	34	44
B. C	32	47	L. G.	32	38
M. C.	31	41	H. H.	31	41
E. B.	31	69	D. J.	31	36
E. C.	30	39	G. B.	28	38
C. W.	26	46	M. T.	26	37
A. L.	23	54	L. B.	24	45
N. C.	19	32	B. T.	22	29
D. M.	18	34	N. H.	20	25
W. B.	16	30	A. G.	16	21
Averages:	35·0	49·5		34·8	38·8

Only two girls in the practised group have failed in the final tests to surpass those in the non-practised group with whom they were originally paired[1]. The average age of the practised group was 12 years 6·4 months and the average age of the non-practised group was 12 years 7·3 months on July 31st. The correlation between the results of the preliminary tests is ·981. The difference between the means is ·2 and its 'probable error' is ·328. The difference, therefore, is not significant, for it is less than its probable error. The correlation between the two series of results in the final tests is ·621. The difference between the means is 10·7, with a 'probable error' of 1·34. The difference, therefore, is highly significant, being 8 to 9 times its probable error[2].

---

[1] One unfortunate slip occurred: E. W. in the non-practised group, who only scored 34 in the preliminary logical tests, was entered as scoring 42 by a clerical error and was paired wrongly with W. M. in the practised group.

[2] The 'probable error' of the difference between the means was calculated from the formula $\cdot 67449 \dfrac{\sqrt{\sigma_1^2+\sigma_2^2-2\sigma_1\sigma_2}}{n^2}$.

The results given above may be shown more compendiously in the following table.

Table II. *Showing, section by section, the Results in Logical Reasoning of the two Groups compared.*

Marks in Preliminary Logic Tests	Practised Group			Non-Practised Group		
	No. of Girls	Preliminary Average	Final Average	No. of Girls	Preliminary Average	Final Average
Over 45	3	52·0	64·7	3	51·7	53·3
40–45	5	42·0	48·2	5*	40·4	36·4
30–40	7	33·1	51·1	7	32·9	39·6
Under 30	5	20·4	39·2	5	21·7	31·4

* This section is not well-balanced, owing to the mark for E. W., which is explained in the penultimate footnote.

A subsidiary interest lies in the improvement shown in the practised function itself, viz., problematic arithmetic. Teachers will hardly require to be told that it was perceptible in the non-practised group and enormous in the practised group. The practised group rose from an average of 16·3 in the preliminary tests to 46·2 in the final tests, the non-practised from 15·8 in the preliminary tests to 21·25 in the final—a differential improvement of 24·4 in favour of the practised group. The following table sets out the results in more detail. Using the same system of grouping as in the preceding table, we have:

Table III. *Showing, section by section, the Results in Problematic Arithmetic of the two Groups compared.*

Marks weighted for difficulty

Practised Group			Non-Practised Group		
No. of Girls	Preliminary Average	Final Average	No. of Girls	Preliminary Average	Final Average
3	23·0	49·7	3	18·3	28·3
5	17·4	48·0	5	16·0	20·0
7	18·0	45·1	7	18·9	24·4
5	8·6	43·8	5	9·6	13·8

V. SUMMARY.

A differential improvement of 24·4 on 16 marks in favour of the group practised in Problematic Arithmetic is accompanied by a differential improvement of 10·5 on 35 marks in Logical Reasoning. In other words a differential improvement of about 150 % in Problematic Arithmetic is accompanied by a differential improvement of about 30 % in Logical Reasoning.

(*Manuscript received* 19 *December*, 1922.)

EXPERIMENTS ON THE ESTIMATE OF DURATION

BY MARY STURT.

Experiment 1 (p. 383).
Experiment 2 (pp. 383–385).
Experiment 3 (pp. 385–387).
Conclusions (pp. 387–388).
Summary (p. 388).

THE plan of these experiments was simple. The subject was asked (*a*) to estimate, in terms of seconds or minutes, the time that a pencil was held up, or (*b*) in some series, to start a stop-watch and to stop it when a certain number of seconds had elapsed. Some lack of precision was involved in method (*a*); but as the times used in this experiment were comparatively long, this was not serious. Three series of experiments were undertaken: (1) a trial series in which *M* and *N* acted alternately as subject and experimenter and method (*a*) was used; (2) a series conducted by the writer alone using method (*b*); and (3) one in which the writer was subject and *O* experimenter, both methods of procedure being used. The times varied from one or two seconds to one minute.

In all three series of experiments the effects of practice were, so far as possible, guarded against. In (1) neither subject knew her own results, though the course of the experiment naturally familiarised her with the different periods. In (2) no special precautions could be taken as the writer was working alone. In (3) the subject had no knowledge at all of her results and was not using the stop-watch for any other purpose at the time.

In all cases the estimates of time were made in sets of from six to eight estimates on each occasion. For each set two measures were calculated: (i) the average percentage error, irrespective of sign, (ii) the percentage ratio of the estimates as a whole to the actual time. Of these two measures (i) shows the general correctness of the series, (ii) shows whether the time as a whole appeared too long or too short, the ratio of course being larger than 100 in the former case and smaller in the latter.

Experiment 1. The results obtained from subject *N* show considerable irregularity both as regards accuracy and the comparative length of the real and apparent time. There is no constant tendency to judge the times as either too long or too short; and there is no sign of improvement with practice.

A larger number of sittings were obtained from subject *M*—in all 15 spread over 16 days. The results are similar to those from *N* except that the irregularities are greater, the average error varying from 60·8 per cent. (series 2) to 6·4 per cent. (series 1) and the ratio from 153 through all degrees to 58. As in *N*'s results, there is no definite practice effect discernible.

Towards the end of this series of experiments the effect of pain on the time estimate was studied. At the beginning of a sitting the subject started to smoke and whenever the experimenter raised the pencil, she brought the lighted end of the cigarette against her hand, keeping it there till the pencil was lowered. The following table gives the results for that sitting (no. xii) and for the four sittings which preceded and followed it in the experiment; the latter were carried out under approximately the same conditions except for the pain.

Table I. Subject *M*.

No. of sitting	x	xi	xii	xiii	xiv
Average percentage error ...	22·7	33·	19·4	36	42·5
No. of estimates with + error...	6	2	3	0	1
No. of estimates with − error...	3	5	3	7	7
No. of estimates correct ...	0	0	1	0	0
Percentage ratio to actual time	105	96	98	68	74

The result is interesting and was quite unexpected. Of the five sets here given, xii (the pain series) has the smallest error and the percentage ratio is nearest to 100. It contains also the only quite correct estimate in the five series. Taking the results of the whole experiment for *M*, xii is the fourth best as regards average error and the percentage ratio is the nearest to 100 in the whole series. It is therefore one of the best sets of estimates and in it the estimated time shows least definite bias in either direction.

Experiment 2. The circumstances in which this series of experiments was conducted were intentionally very varied. The series falls into three groups, (*a*) comfort, (*b*) discomfort, (*c*) in the railway train, which are *not* mutually exclusive. The conditions considered comfortable were—(*a*) in bed at night before settling down to go to sleep, (*b*) in a room alone during the day, (*c*) in the train when the experimenter was in a compartment which was quiet and sufficiently airy. Discomfort included (*a*) the prick of a pin, (*b*) considerable hunger when waiting for a meal, and (*c*) a stuffy railway carriage with a screaming baby in it. It will be observed that the experiments performed in the train include conditions both of comfort and discomfort. In certain of the experiments under comfortable conditions, an attempt was made to fix the attention by listening to (without counting) the ticking of a clock in the room. In all, 32 sets of estimates were made, the experiment lasting over 12 days. On many occasions more than one set of estimates was made at a sitting, the subject leaving only long enough between the sets to prevent fatigue or to make changes in the conditions. Except in special cases the subject allowed her thoughts to wander. When the experiments were carried out in the train she usually looked out of the window.

Dividing the results into three groups, Comfort, Discomfort, and Train, they are as follows:

Table II.

	Total no. of series	Average percentage error	Percentage ratio of estimated to actual time
Comfort	16 (excluding those in train)	25·6	91
Discomfort	7 (" " ")	24·4	89
Train	9	28·7	106

The Comfort and Discomfort results are practically identical, the tendency being for the subject to find time pass too quickly. The results for experiments done in the train are markedly different. The average error is rather larger, and the ratio shows that the subject found the time too long. That this result is not due to feelings of comfort or discomfort is shown by the fact that the train series included conditions of both kinds, and also by the results of certain comfort and discomfort series which were expressly arranged to be closely parallel to each other, and in which the results were practically identical.

Subjectively the distinguishing mark of the train experience was that the subject was continually entertained with the scenery or the movements of her fellow travellers. On the other occasions when carrying out the experiments she was alone and though her thoughts wandered, they did so without much external provocation.

In some of the experiments, as stated above, the subject attempted to keep her thoughts fixed on the ticking of a clock. This was always done under comfortable circumstances and the series was made parallel to other comfortable series, during which the mind was allowed to wander. The results of comparing five series when the attention was thus fixed with five parallel ones when it was not are:

Table III.

	Average percentage error	No. of cases with + error	 with − error	 correct	Percentage ratio of estimated to actual time
Attention fixed	26·7	19	6	5	79
Attention free	26	13	13	4	85

In these results the average error is approximately the same; but the ratio is lower in the series with the attention fixed, thus showing that the time seemed shorter. Now the subject had a strong objection to these experiments. They were distinctly more boring than usual and the attempt to attend to the ticking and to exclude irrelevant thoughts involved unpleasant effort. At the same time, in so far as this attempt was successful, the 'amount of thought' during the interval of time was decreased.

The extreme percentage ratios are: 'Attention fixed' = 79, 'In the train' = 106; while the ratios for Attention free, Comfort and Discomfort series are intermediate. This means that time seemed shortest when there was least range of mental activity, longest in the train when there was most, and intermediate in other circumstances. The pleasantness or unpleasantness of the experience seemed to have little effect.

Experiment 3. In the interval between this and the preceding experiment, the writer had worked out the results of experiment 2, but on a basis quite different from that given above. She did not, therefore, start the following experiment with any definite theory prejudicing her mind.

Two sets of experiments were now carried out each day, one in the morning about 10.30, the other in the evening about 11 o'clock. The morning experiment consisted of three series—(1) the procedure being as in experiment 1, (2) with procedure as in experiment 2 (except for the fact that the subject did not record her own times), (3) as in (2), except that the subject was reading aloud from a book (a translation of Gogol's *Taras Bulba*) throughout the whole experiment. In the evening two sets were performed—1 *a* and 2 *a*—the procedure being the same as for (1) and (2) above, except that the subject was always smoking.

The results are as follows:

Table IV.

Morning Experiments. Subject *M.*

Series	A	B	C	D	E	Av. of all series
Average percentage error:						
Series 1	25·5	33·3	24·7	35·1	34·3	30·5
2	17	39·8	43·8	33	63·6	39·4
3	19	31·1	37·1	18	43	29·6
Percentage ratio of estimated to actual time:						
Series 1	83	67	75	65	66	71
2	83	73	65	69	45	67
3	111	108	111	94	58	96

In these results the average error is approximately the same for series 1 and 3. It is highest for series 2. This shows that the grave distraction incident to reading aloud had no prejudicial effect on the power to estimate time. The ratios, on the other hand, show that reading increased the apparent length of the time. The whole tendency in these experiments was to judge time too short. The result of reading is to make the estimated time approximately the same as the real time.

These results are in accord with those of experiment 2. There the movement of the train made the time seem long; here reading with its consequent production of ideas has the same effect.

The evening experiments were intended to test once again the effect of pain on estimates of time. As in experiment 1, the pain was a fairly severe burn from a lighted cigarette. In the following table the pain series is K.

As in the morning experiments, the procedure of 2 *a* gives larger errors than that of 1 *a*, but the difference is not so marked. The ratios are closely similar, the time always appearing shorter than in reality.

Table V.

Evening Experiments. Subject *M.*

Series	F	G	H	K	M	Av. of all series
Average percentage errors:						
Series 1 *a*	39	16	30·6	38	20·6	28·8
2 *a*	21·5	23·6	16	41·6	59·5	32·4
Percentage ratio of estimated to actual time:						
Series 1 *a*	61	94	100	62	79	79·2
2 *a*	97	91	84	58	40	75

The pain series—K—is distinguished by having the second largest error in the series, but this error is always in the same sense—negative.

The times therefore appeared much too short. In fact the ratio for tl series is almost the lowest (cf. experiment 1) in the whole researc This apparent shortening of the time seems to be due to a fixation of t attention comparable to, but more extreme than, that in the series wh the subject listened to a clock ticking. The pain series in experiment 1 bea the note: "I think I did these estimates well as my attention was fixec

CONCLUSIONS.

These experiments confirm the generally accepted statement tl in estimating time we rely on the amount of mental content experienc during that time. Time which has been filled by many thoughts appe longer, whereas time occupied by few thoughts appears shorter. In pai cular, time spent in the train or in reading seems longer, time when t attention is immobilised by a ticking clock, or yet more forcibly by pa seems shorter. On one occasion in the morning series of experiment 3, t subject felt that she had given unusually correct estimates. Her mi was clear and empty; no irrelevant ideas intruded, and the idea of ti was kept clearly before the mind. This series was E 2. It will be se (Table IV) that the error is the largest in the series, and the ratio smallest. Thus on this occasion time passed at its fastest for that exp ment.

It is also generally accepted that the apparent length of time depe in large measure on the pleasantness or unpleasantness of the experie contained in it. As regards moderately short periods of pain the exp mental evidence above adduced is definitely against this assumpti for longer periods the effect of acute pain seems to be to destroy power to estimate time. After an attack of toothache or neuralgia sufferer would find it hard to tell for himself whether the pain had las one hour or five. The concentration of attention due to the pain se to render impossible the reference of such experience to a conventic time scheme.

A crucial case is that of waiting for a friend who fails to keep appointment. The generally accepted analysis of the situation is that has nothing to do, therefore the experience is unpleasant, and there it seems long. The time undoubtedly seems long and the experienc unpleasant, but it is not certain that these facts are causally connec The unpleasant feeling tone may be due to a thwarted conation. long to be away to our destination, to begin our talk. Our pride is l by being thus kept waiting through another's carelessness; we worried lest we shall miss our train or get no supper. The apparent ler of time seems to have another cause. So far are our minds from bein and unoccupied that they are a very frenzy of whirling thoughts. y swarm with words of reproach, with anxious speculations as to the ›e of delay, with plans for the future, etc. We will not let ourselves l, but turn the pages of the books on the nearest bookstall, trying by greatest possible number of stimulations to distract our minds— so we increase the time of our torment! The apparent length of time ıt in waiting is due rather to the number of impressions we force n ourselves than to the unpleasantness of the experience.

This principle, that apparent length of time depends on the amount nental content, is also in accordance with general experience. A od of travel may seem long in experience as well as in retrospect. nty-four hours which the writer spent in Paris seemed, and seem, ıt the length of an ordinary week; and it was a pleasant experience. the other hand, days of joy when the attention is focussed on a single ved object fly fast. The case of a woman—a sufferer from insomnia— ıoted by Binet[1], whose fear of not getting her five hours' sleep made nights apparently pass in a flash. We can also explain on the same s Janet's[2] observation that years seem to pass more quickly with easing age. In old age, it takes longer to arrive at an idea, longer to than in youth. Therefore the years actually hold less for the old. ı is an easier explanation than Janet's own, and perhaps better than ıes's[3].

With this theory regarding time estimates we may compare two well-wn facts: (*a*) when a person is asked to estimate the relative length of historical periods—*e.g.* if asked which was the longest time, from the invasion of England by the Romans to the Norman Conquest or from Norman Conquest to the present day—he is likely to judge that period e the longer about which he knows the most. Abundance of content give the impression that the time is long; (*b*) the illusion of filled and ty space. Here in the realm of vision we have the same principle— space is filled it appears larger than one which is empty.

SUMMARY.

l. The estimate of the duration of a moderately short period of ı seems to depend mainly on the 'amount' of mental content during period. This is in accord with certain other generally recognised facts.

l. The unpleasantness of a mental content does not increase the arent duration of time.

Arch. de Psychol. 1903. [2] *Rev. philos.* vol. III. [3] *Principles of Psychology*, vol. I.

(*Manuscript received* 16 *January* 1923.)

AN APPLICATION OF AMERICAN ARMY INTELLIGENCE TESTS

By S. BOWIE.

I. *Introduction* (p. 389).
II. *The tests* (p. 390).
III. *Results* (pp. 390–394):
  (*a*) *Whole examination.*
  (*b*) *Comparison with school roll order.*
  (*c*) *Comparison with school order in English and mathematics.*
  (*d*) *Performance of each set in each test.*
  (*e*) *Performance of each age group in each test.*
IV. *Comparison with results of external examinations* (p. 394).
  (*a*) *University of Durham school certificate.*
  (*b*) *Durham matriculation.*
V. *Inter-correlation of the tests* (pp. 394, 395).
VI. *Criticism of the tests as a whole* (pp. 395–397).

I. Introduction.

The object of this investigation, carried out at Newcastle-on-Tyne Royal Grammar School, was to see if there existed any correlation between the pupils' achievements in a particular set of group tests for intelligence and in formal school examinations. Seventy boys (the Lower Fifth Form and three Fourth Forms) were examined. They were grouped into 'sets'—A_3, A_4, A_5, A_6—for teaching, individual boys not necessarily being in the same set for all subjects. The boys in A_3 and A_4 were preparing for the University of Durham School Certificate, those in A_5 for the Durham Matriculation Examination. Briefly, they might be described as follows:

(Science Sets.) A_3—young, bright boys, above the average;
A_4—older; average examination age; but dull;
A_5—slowly developing boys;
A_6—mixed residue, containing fairly wide extremes.

A School Roll order, to which future references are made, is prepared each term. It is based upon the marks obtained by each pupil in the several subjects of his curriculum, duly weighted by a 'bonus' according to the time he spends and the set he has reached, in each.

II. THE TESTS.

The tests were, with one or two minor alterations, substantially those constituting Form 5 alpha[1]. As cyclostyled copies of the tests are not so quickly legible as printed copies, the time allowed for each test was increased (usually doubled), except in the case of the first test, where the time limits set forth in the examiner's guide were strictly followed.

The examination was held the first week after the Easter vacation, 1921. It was at the end of this term that the boys of A_3, A_4, A_5 were to take their respective external examinations.

The system of marking adopted was identical with that detailed in the above-mentioned manual.

III. RESULTS.

(*a*) *Whole examination.* The marks obtained ranged from 195 down to 96, out of a possible 212. According to the American system of classification, of the 70 boys examined 57 would be graded A, 12 would be graded B, and the remaining 1 C +. To the writer, from personal knowledge, this classification seems unduly favourable; probably the time allowance made for inferior legibility was too generous.

For purposes of comparison, A_3, and A_4 (Science) were regarded as one group. A_5 and A_6 were already self-contained entities. Before the end of term, however, seven boys of A_5 had decided not to enter for the matriculation examination. Correlation factors have therefore been calculated for the whole set and for that portion of it which actually took that examination (called A_{5m}). This latter is to facilitate comparisons with the results of the external examination.

(*b*) *Comparison with school roll order.* The following correlation coefficients (using Pearson's Rank Formula) were obtained between intelligence order and the respective roll orders at Easter and Summer.

Table I.

Set	No. of boys	Easter	Summer
A_3, A_4	26	·54 ± ·09	·54 ± ·09
A_5	24	·63 ± ·09	·51 ± ·11
A_{5m}	17	·73 ± ·08	·56 ± ·11
A_6	20	·23 ± ·15	·25 ± ·15
All sets	70	·43 ± 07	48 ± 06

---

[1] Yoakum and Yerkes, pp. 206–219.

There is a fair degree of correlation, particularly in the upper sets. The low correlation in the case of A_6 is probably to be explained, in part at least, by the heterogeneity of the boys comprising it. The age varied from 13 years 11 months to 16 years 10 months; and as has already been explained, this set constitutes for some a sort of educational repository above which they do not rise because they attain a leaving age, and for others, it means a year of preparation. Generally speaking, the older boys were duller than the younger ones at the test, but were even more so in their work in form.

The unsteadiness which characterized the school work of A_5 probably accounts for the low correlation in Summer. Having early in the term decided not to take the matriculation examination, many had slackened visibly. In A_{5m}, there were only two cases of a difference of more than four places between the Easter and Summer orders; but with small numbers these are sufficient to affect the coefficients considerably.

It will be noticed that A_3 and A_4 remained steady.

(*c*) *Comparison with school order in English and mathematics.* It has been held by some that intelligence tests measure literary abilities and little else. To test this view, the results of the whole examination have been correlated with the boys' positions in their respective English sets (Table II *a*). Test IV (Synonyms and Antonyms), Test V (Disarranged Sentences) and Test VII (Analogies) are distinctly literary in their requirements. The marks of these three tests were therefore segregated, arranged in order of merit for each of the four English sets and correlated with the Easter English order (Table II *b*).

Table II.

English set	(*a*) School English position with whole of Intelligence tests	(*b*) School English position with 'Literary' tests	(*c*) School position with whole of Intelligence tests
V_2	·68	·84	·74
IV_1	·07	−·01	·41
IV_2	·57	·56	·63
IV_3	·39	·37	·23

If the tests really measure 'literary ability and little else,' the correlation coefficients in column (*b*) should in every case exceed the corresponding coefficients in column (*a*). An examination of the table shows that only V_2 conforms with this requirement. IV_1 holds a most anomalous position on account of its extraordinary coefficient. These results, therefore, do not, on the whole, support the above-mentioned opinion.

Those two tests which make purely mathematical demands were similarly analysed and correlated with the Easter Mathematical order (Table III *a*).

Table III.

Mathematics set	(*a*) School Mathematics position with whole of Intelligence tests	(*b*) School Mathematics position with 'Mathematical' tests
A_3	·08	·03
A_4	·00	−·17
A_5	·23	·54
A_6	·25	·10

A comparison of columns (*a*), Tables II and III, shows that the correlation between English and Intelligence is much higher than between Mathematics and Intelligence.

It will be seen that in column (*b*) only one (A_5) shows any appreciable correlation. The possible marks for the two tests combined was only 40. This meant that for small differences in marks obtained, relatively large differences in rank accrued—for instance, the differences in marks obtained by top and bottom boys of two sets were 11 and 12 respectively, but these represented respective rank differences of 11 and 23 places.

(*d*) *Performance of each set in each test.* Table IV shows the average marks obtained in the whole examination and in each individual test by each of the various sets.

Table IV.

Set	I Instructions	II Arithmetic	III Judgment	IV Synonyms and Antonyms	V Disarranged Sentences	VI Number series completion	VII Analogies	VIII General Information	Total
A_3	10·7	15·7	12·0	31·4	19·7	14·7	33·7	28·5	166·2
A_4	9·5	14·0	10·5	26·8	17·8	12·9	31·3	25·7	148·5
A_5	9·9	15·4	13·0	30·5	18·5	14·6	31·8	28·2	163·4
A_{5m}	10·1	15·4	13·0	31·8	18·2	14·4	32·2	29·6	164·7
A_6	9·7	13·5	11·7	25·3	18·0	13·5	30·6	27·2	149·5

It will be noted that A_3 (Science) in the majority of cases (six out of nine) leads the way; and that in every case it was easily ahead of A_4.

(*e*) *Performance of each age group in each test.* The results of the tests were then examined from a different view point—that of age, irrespective of school standing (but see summarised estimate on p. 389). The ages ranged from 13 years 7 months to an extreme case of 17 years 11 months. These were grouped at 12 months' intervals; but as the number of boys in the 'below 14' and 'over 17' groups was only two in each case, these were joined on to the respectively adjacent groups. The distribution was a fairly regular one.

The average mark for the examination and for each individual test was calculated for the several groups; thus the appended Table V was compiled.

Table V

No of Cases	Age	Aver. Age	I Instructions	II Arithmetic	III Judgment	IV Synonyms and Antonyms	V Disarranged Sentences	VI Number series completion	VII Analogies	VIII General Information	Total
17	Up to 15	14.6	10·2	15·4	11·9	30·2	19·4	14·1	31·8	28·6	161·5
25	15–16	15.8	9·7	14·8	11·8	28·2	18·5	14·0	32·6	26·8	156·3
28	Over 16	16.6	10·0	14·1	12·1	27·8	17·8	13·9	30·9	27·8	154·5

The general superiority of the younger portion becomes at once apparent.

The reasons for the marked superiority of A_3 (Science) over A_4 become now more obvious. A_3 contained only three boys whose age was over 15.0, and the average age of the form was only 14 years 9 months. It consisted, therefore, of young boys who had been accelerated through the school because of their superior attainments. A_4, the average age of which was 15 years 7 months, consisted of older boys retarded because of lower ability. Of the 24 boys whose age was below 15 years 7 months, that is, those who showed themselves in this examination as a group of superior intelligence, 14 were scholarship holders, and of these, 10 were placed in the first 17. The aim in selecting boys to become holders of free scholarships tenable at this school is, if possible, "to single out those boys who have marked abilities in some special direction, and whose mental age is two years in advance of their chronological age"[1]. The scholars under present consideration were elected to scholarships three or four years ago, and about half of them were in A_3. This seems to be evidence, therefore, that the efforts to achieve the above ideal have met with a measure of success.

If the American classification of intelligence, already referred to

---

[1] Laws and Bowie, "Intelligence Tests in Junior Scholarships," *J. of Exp. Ped.* VI. 3.

(p. 390) be assumed as applicable to the present investigation, then the average age for those of Group A is 15 years 6 months, and that for those of Group B is 16 years 0½ month.

IV. COMPARISON WITH RESULTS OF EXTERNAL EXAMINATIONS.

(*a*) *University of Durham school certificate—July* 1921. On comparing the intelligence order of the candidates with their performances in this examination (pass or fail), the following facts stand out:

(1) The first eight in intelligence order all passed successfully;
(2) The last six in intelligence order all failed;
(3) The middle portion (9th to 19th) fluctuated considerably;
(4) A_3 more or less lived up to its reputation (11 passed; 2 failed);
(5) Of A_4 (5 passed; 8 failed).

It would seem from this that the tests do differentiate the markedly superior boys and also the markedly inferior ones, but that they are of little assistance in arranging the 'middle class.'

Thanks to the courtesy of Prof. G. H. Thomson, who was allowed access to the examination marks for the purpose, it is possible to quote the following correlation coefficients, Pearson's Rank Formula being used.

(1) Intelligence Tests (whole) with School Certificate ... ... Mathematics $r = \cdot 27 \pm \cdot 1$
(2) ,, ,, ,, English $r = \cdot 47 \pm \cdot 1$
(3) ,, ,, ,, (whole) $r = \cdot 60 \pm \cdot 1$

It will be noticed that the test results correlate more closely with English than with mathematics; but less highly with these than with the results of the whole examination (cf. Table II, columns *a* and *c*).

Another point of interest is that the correlation coefficient (3) is a higher one than those given in Table I. Is this because of the absence of bonuses?

(*b*) *Durham matriculation.* In this examination, 80 per cent. of total entrants failed. Of A_{5m}, only two passed in July, and two were successful at the second attempt in September. It is worthy of note, however, that three of the four successful boys appear in the first four in the intelligence order.

V. INTER-CORRELATIONS OF THE TESTS.

The correlation coefficients in Table VI were obtained by using Pearson's Product-Moment Formula. This table is for the whole group of 70 boys.

Table VI.

Test	No.	V	VII	III	VIII	VI	II	IV	I	Total
Disarranged Sentences...	V		·54	·50	·46	·43	·36	·35	·31	[·20]
Analogies	VII	·54		·49	50	·41	·30	·28	·41	[·21]
Judgment	III	·50	·49		·44	·42	·33	·24	[·23]	[·19]
General Information ...	VIII	·46	·50	·44		[·16]	·27	·42	[·23]	[·22]
No. Series Completion ...	VI	·43	·41	·42	[·16]		·40	·33	[·21]	[·16]
Arithmetical	II	·36	·30	·33	·27	·40		·28	·29	[·22]
Synonyms and Antonyms	IV	·35	·28	·24	·42	·33	·28		·30	[·19]
Instructions	I	[·21]	·41	[·23]	[·23]	·31	·29	30		[·10]
Total		[·20]	[·21]	[·19]	[·22]	[·16]	[·22]	[·19]	[·10]	

[r] indicates that r is less than thrice the probable error.
When r = between ·16 and ·23, p.e. is ±·08.
r = between ·24 and ·36, p.e. is ±·07.
r = between ·37 and ·54, p.e. is ±·06.

It will be noticed that Test II correlates most closely with Test VI (both mathematical); Test III with Test V (both involving judgment); Test V with Test VII (both literary).

VI. CRITICISM OF THE TESTS AS A WHOLE.

Upon the tests taken together as a complete examination, the following criticisms may be offered:

1. They contain a preponderance of English tests. To guard against undue over-weighting by English marks, other tests should for school purposes be marked more liberally, so as to give those boys of pronounced mathematical ability (accompanied by inferiority in 'English') an equitable chance.

2. Tests II and VIII are not, in the writer's opinion, particularly suited for use in schools; the former because it is purely a test in mental arithmetic and admits coaching and is therefore not satisfactory mathe-

matically; the latter because it is a test of general information which requires little or no thinking. While the test is probably an admirable one for differentiating large masses of people into those who are well-informed on general topics and those who are not, it does not seem to fulfil any useful psychological purpose in schools. The fact that the set which obtained highest average marks was that with the highest average age lends support to the view that this test probably finds its most judicious application in after-school ages.

3. It is assumed that the marking of Tests IV (Synonyms and Antonyms), and V (Disarranged Sentences), on the (Right—Wrong) basis is to neutralise any too successful application of the invitation to "guess if you can't be sure[1]." It is more than possible that if not deliberately invited to guess, many of the examinees would be restrained by native caution from making the plunge—and might profit thereby.

4. Schoolmasters might classify their pupils into the following categories:

(i) Those who have real ability and who work;
(ii) Those who have real ability and do not work;
(iii) Those who have little ability and do work—*i.e.* the 'plodders';
(iv) Those who have little ability and do not work—*i.e.* the 'shirkers.'

In these tests (i) and (iv) invariably find their respective levels, but groups (i) and (ii) coalesce, as do, unfortunately, groups (iii) and (iv). The novelty of the tests appeals to those of (ii)—and they frequently betray themselves. A glaring example of this was to be observed in the case of A_6. The boy who was easily top of the intelligence order was much more easily bottom of the school order.

The slower, more deliberate type (iii), popularly supposed to make our national 'character' abroad as the hard-headed, calculating Britisher, is quite cut out in this intelligence examination. Possibly the introduction of a test on the lines of Burt's Reasoning Tests, with an ample time allowance, would do much to rectify things. It would certainly help those of this nature to rise above those of group (iv).

The conclusions arrived at in this paper are—because of the small number of cases examined—necessarily tentative. Some correlation has been shown to exist between performances in the tests and those in more formal examinations, but with the limitations mentioned in the last paragraph. These shortcomings should make one hesitate to acclaim the feasibility of replacing formal examinations by the intelligence tests here employed; the more conservative view would be to say that they supply a useful confirmation and sometimes a corrective, if collaborated with such examinations.

---

[1] Yoakum and Yerkes, p. 64.

There is not sufficient evidence of a conclusive nature to refute or to confirm the statement that these tests measure almost solely 'literary' abilities.

The correlation coefficients between the tests and school orders are generally sufficiently high to justify the conclusion that these tests do measure some form of intelligence which is of value in school work.

I desire, in conclusion, to express my indebtedness to Major J. Talbot, for facilitating the conduction of the investigation; to Prof. G. H. Thomson, for his co-operation in connexion with the results of the external examination; and to my colleague, Mr A. R. Laws, for his invaluable criticism and advice.

(*Manuscript received* 8 *June* 1922.)

THE INFLUENCE OF SUBJECTIVE PREFERENCE ON MEMORY[1].

BY CHARLES FOX.

Do we remember better what we like better? Such is undoubtedly the popular view, and it is the object of this paper to provide some experimental evidence bearing on the question. Comparisons have frequently been made between different methods of learning, with regard to their efficiency for immediate or delayed recall. The effect on memory, however, of subjective preference for the material learnt, has been strangely overlooked by experimenters, owing, no doubt, to the difficulty of arriving at a numerical measure. Theoretical writers, on the other hand, lay the greatest stress on this subjective factor as determining both the power of retention and the ability to recall what has been learnt. Thus, Stout (*Manual,* Book IV, Ch. III, par. 2, 1913) says—"The rapidity with which the power of recalling is acquired depends to a large extent on the keenness of the interest attaching to the original experience," and again,—"Differences in the length of time during which the power of recall is retained also depend largely on interest." So much stress does he place on interest as determining memory that he does not scruple to say that "it is doubtful how far congenital constitution can favour memory without favouring interest." It must be borne in mind that Stout means by interest both conation and feeling-attitude in intimate and inseparable union, which is precisely what I understand the term "subjective preference" to imply. If Stout's view, as expressed in the above final quotation, is pressed to its logical conclusion, it would appear to be indistinguishable from Bergson's belief that pure memory is entirely a mental process. The implications of such a position are examined at the end of this paper.

The negative aspect of the question we are considering has been dealt with very thoroughly by Freud who maintains that all forgetfulness is due to repression. He says—"The forgetting of impressions and experiences shews the working of the tendency to ward off from memory that which is unpleasant[2]." It is worthy of note that all cases of normal forgetting considered by Freud are concerned with isolated impressions and that he does not consider serial memory, or the obliviscence of organized groups of ideas. In the particular case of forgetting names of people, he thinks that these are always associated with a 'personal complex' which is apt to arouse strong or painful emotions. In short we forget what we do not like.

---

[1] Read to the British Psychological Society, 11 November, 1922.

[2] See *Introductory Lectures on Psycho-Analysis.* Fourth Lecture (Eng. trans. 1922).

Now it is notorious that we often acutely remember what we consciously dislike and would much prefer to forget. On the other hand, we frequently fail to remember what we desire strongly to retain. A striking instance of the failure to recall, accompanied by a strong desire to remember, occurred to a pupil of mine, who met the following sentence in a German book, "Stunden der Not vergiss, doch was sie dich lehrten vergiss nicht." The idea embodied in the quotation appealed to him strongly, as it expressed concisely and neatly a thought of which he approved but had never quite formulated. He repeated the lines a few times in order to remember them but found shortly afterwards that not only had he completely forgotten them, but he had forgotten the idea itself. After an interval he again made the attempt to learn the lines with the same result. All he could remember was that a contrast was expressed, but not the nature of the contrast. A prolonged examination revealed the fact that during the war, in which he had seen a good deal of fighting, the whole of his platoon except himself was killed during a night attack; and just before he himself was hit in the head, a particularly distressing incident had occurred involving his dearest friend. The shock he sustained brought about amnesia for the incident referred to and some other matters connected therewith, which lasted for two and a half years. It was evident during the examination that he had connected the words "Stunden der Not" with the terrible incident of the night attack; to which, in fact, they were peculiarly appropriate. The connexion so made was the cause of his inability to memorise the sentence. In this case the psycho-analytic view seems justified, namely that a conscious intention to remember (or forget) is powerless to counteract an unconscious resistance. Freud extends this theory to all normal cases of forgetting or faulty reproduction of words, and maintains that the forgotten or distorted material has some associative connexion with an unconscious stream of thought[1].

So much for the negative aspect of the question. The positive side must be approached by a different method, which is described in the following experimental enquiry. The investigation was undertaken primarily in order to compare the relative advantage of two methods of memorising, in so far as they were affected by the preference of the persons for what was learnt.

---

[1] See *Psychopathology of Everyday Life*, Chs. II, III and XII (Eng. trans.), 1921.

Twenty-four men took part in the experiment, their ages ranging from 22 years to 38½ years, with an average of 26 years 3 months. They were all graduates, mostly in honours, being members of a practical class in psychology and they displayed great keenness on the experiment; five of them were men of considerable literary judgment. The material to be learnt consisted of four sonnets from Hardy's *Collected Poems* named "She to Him." The selection was determined partly by the sonnet form, which is useful in such investigations owing to the rigidity of its construction which provides comparable tasks, and partly by the fact that these sonnets have exactly the same 'colour' and deal with the same order of ideas. Neither the name of the author nor the title of the poem was given, and on being asked to guess who composed them nearly all stated that they were written by a woman.

Each subject was given two of the sonnets to learn in a prescribed manner. The first was learnt by a 'mixed' method according to the following plan, which insured that each line was repeated exactly eight times:

	Plan of repetitions					Total
First quatrain	4		2		2	8
Second quatrain		4				8
Sestette				6		8

The second sonnet was learnt by the so-called 'entire' method, being read through from beginning to end eight times; so that each line in both sonnets was repeated the same number of times. In the former case, however, as the plan shews, there were several irrelevant associations formed during the course of learning between the beginning and end of each quatrain and sestette which, theoretically, should interfere with the process of recall.

After each sonnet had been repeated in the way described, a couple of minutes' rest was taken, and then the accuracy of learning was tested. Each subject heard his neighbour who had repeated a different sonnet. A score was kept by recording the number of times that the person had to be prompted in order to repeat the poem; which serves as an index to the completeness of the learning. One minute only was allowed at any point of hesitation, at the end of which the person was prompted; and every prompt was counted, even if it consisted only of a preposition or article. This rigid method explains the large numbers given later.

Everybody who has experimented on memory is well aware that there are certain places where different persons find exceptional difficulty in making associations. No explanation has been so far given of these 'refractory points,' but their existence calls in question the accuracy of any method of determining the completeness of what has been learnt, which relies on counting the number of repetitions which are required to learn a passage, or to learn a series of nonsense syllables, by heart. All the extra repetitions necessary to break down the resistance at the refractory points are unnecessary as far as the rest of the passage is concerned, which is therefore overlearnt. Hence there is always considerable doubt in the comparative interpretation of results when the efficacy of a learning method is calculated by the number of repetitions necessary to ensure perfect reproduction. The prompting method avoids this difficulty by fixing the number of repetitions in advance and by not insisting on complete recall. In this way, the refractory points do not affect the other associations, but all are treated alike.

Only two subjects learnt one of the sonnets sufficiently well not to require to be prompted, and in both cases by the 'entire' method; by the 'mixed' method they required prompting 11 and 12 times respectively. The average number of promptings for the twenty-four subjects for learning by the 'entire' method was 40 and for the 'mixed' method 38. It would appear, then, that for this group of men, taken as a whole, there is very little to choose between the efficacy of the two methods; the difference between the numbers being insignificant.

The second part of the experiment was carried out exactly one week later. In the intervening period the subjects were told to avoid thinking of the poems as far as possible, and the instruction was successfully followed. At the end of seven days they were allowed to read each sonnet through once, from beginning to end, and one minute later the amount of retention was determined, exactly as before. It should be added that the subjects worked in two groups of twelve, on different afternoons, but all the conditions as to time intervals, etc., were preserved.

It was found that, on the average, for the sonnet learnt originally by the 'entire' method 35 promptings were necessary one week later; for that learnt by the 'mixed' method 33. Here again there seems to be no significant difference between the figures. Both methods, then, shew no difference either for immediate memory or for delayed recall. The sonnets appear to be remembered without any appreciable loss over the week; the diminution of the number of promptings may be due to the final reading. It is interesting to note that the two subjects, who required no prompting the preceding week by the 'entire' method, required none on the second trial; and whereas they had to be prompted 11 and 12 times on the 'mixed' method, they only needed 2 and 0 respectively on this occasion.

Immediately after the final record was made, each was asked to state which of the two sonnets he preferred. Nineteen subjects expressed a decided preference for one or the other; one liked both equally well; and four disliked both strongly, either because they did not appreciate them, or else because they thought them nonsense. The man who liked both equally well had taken high honours in English; those who had no preference had graduated in Mathematics or Science and stated that they frankly disliked or were bored with verse of all kinds.

If the different subjects are classified in groups according to the sonnets they prefer, the effect of subjective preference on memory can be investigated.

Eight subjects preferred the sonnet which they had learnt by the 'mixed' method. In their case, on the average, 26 promptings were necessary for the preferred sonnet for immediate recall, and 18 for delayed recall. For the other sonnet they needed 40 and 33 promptings respectively. The preferred poem is, therefore, learnt better and retained better than the other.

Eleven subjects preferred the sonnet they learned by the 'entire' method. On the average, 32 promptings were required for immediate recall and 30 for delayed recall for the preferred sonnet; for the other 43 and 40 respectively were necessary. Just as with the former group the preferred poem is better learnt, and for a week at least better retained.

These results are more clearly seen when exhibited in tabular form, where the average numbers for the group of five subjects who had no preference are given for the sake of comparison. The figures in heavy type in every case are those for the preferred sonnet:

	Immediate recall		Delayed recall	
Learnt by	I 'Mixed' method	II 'Entire' method	I 'Mixed' method	II 'Entire' method
24 subjects				
No. of prompts	38	40	33	35
8 subjects (preferring Sonnet I)				
No. of prompts	**26**	40	**18**	33
11 subjects (preferring Sonnet II)				
No. of prompts	43	**32**	40	**30**
5 subjects (no preference)				
No. of prompts	47	57	44	51

These figures seem to suggest that it is not the method of learning which affects immediate memory or retention, but rather the subjective preference for what is learnt. Statements have been made in conflict with this opinion, such as that of Myers[1], who says that the mixed method, "although more economical as regards immediate memory, is surpassed by the entire method in respect of retention." Such a view appears to be the result of taking averages from disparate groups or for dissimilar material, where the preferences cancel each other in the aggregate.

It follows from our investigation, that any experimental evidence about memory which does not take the subjective factor into account must be rejected. For if the experiments are valid, it would appear that subjective preference is a *vera causa*. The objection will at once be raised that "there is no cause or effect in nature; nature has but an individual existence; nature simply *is*[2]"; and cause and effect are convenient fictions serving the purpose of economy of thought. The concept of cause, we are told, is now replaced by the concept of function, and with the abolition of the causal concept the idea of activity must also be abandoned.

However well founded this view may be in the realm of physical science, it is impossible to dispense with the notion of subjective activity in psychology. If the psychologist finds it necessary to take into account the fact of thinking at all, and none except the behaviourist fails to do so, he is equally bound to postulate the activity of thought[3]. Neglect of this consideration is due to a physiological bias in psychology, by which function is regarded as determined by structure instead of *vice versa*. Mental structure is, however, simply crystallized function. From the psychological standpoint function is always prior, as we see in dealing with habit and heredity regarded as mental phenomena.

Mental activity has been manifested wherever we find evidence of subjective preference, the importance of which in the whole field of psychology has been most consistently demonstrated by Ward. The experiments above described lend support to his view that "there is no pure passivity in experience; and even the association of ideas is determined, not mechanically, but by subjective selection and interest[4]." Subjective selection, the efficacy of which is emphasized in this quotation is but another name for subjective preference.

---

[1] *Text book of Experimental Psychology*, Ch. XIII, p. 165, 1911.

[2] Mach, *Science of Mechanics*, Ch. IV, Sect. iv.

[3] See James, *Principles*, Vol. II, p. 571. Also a recent book by Drever, *An Introduction to the Psychology of Education*, Chs. III and IV, 1922. This author, however, strangely thinks he is adopting the behaviourist standpoint.

[4] *Naturalism and Agnosticism*, Vol. II, p. 220, 1899.

We have seen that memory, as evinced both by immediate and delayed recall, is more efficient in those cases where there is a distinct subjective preference for what is learnt. The result was not anticipated by the subjects, who were often surprised to find which of the sonnets they knew better. Preference exerted a real effect on the memory process which persisted for one week at least. In learning by the 'mixed' method, irrelevant associations are introduced which ought, theoretically, to make the memorising less effective; and this expectation was realised in the case of those who preferred the sonnet learnt by the 'entire' method. The interference of the unnecessary associations was, however, more than counterbalanced by the effect of subjective selection in the case of those who preferred the poem learnt by the 'mixed' method. Diversity or similarity in the mode of presentation is, therefore, not the important factor in learning or recall.

Nothing can well be more 'subjectively subjective' than subjective preference, and this has been shewn to produce objective mental effects in the greater efficiency of memory; so that we seem to be justified in refusing to abandon the idea of causal efficacy in the realm of mind.

THE INFLUENCE OF FEELING ON MEMORY

BY A. WOHLGEMUTH.

I. *Previous experiments* (pp. 405–410).
II. *Observations thereon* (pp. 410–413).
III. *Own experiments and conclusions* (pp. 413–416).

I.

THE statement that unpleasant experiences are more easily forgotten than pleasant ones is often found in psycho-analytic literature. Whether this is true or not can, I believe, only be settled by experimental psychological investigation; psycho-analysis does not, and cannot, furnish a scientific proof one way or the other. Nevertheless the above statement is postulated as a fact, and I have come across the assertion that "the fact itself is beyond dispute, and has not been questioned by anyone who has seriously investigated the phenomenon, either experimentally or clinically."

Now what is the experimental evidence? I will take it, as far as it is known to me, in chronological order.

In 1898 F. W. Colgrave[1] made use of the questionnaire-method when investigating the phenomena of memory. Question 8 ran: "Do you recall pleasant or unpleasant experiences better?" Apart from the fact that this question is liable to several interpretations, the method itself in this case has little scientific value, for one can obtain from the person questioned nothing but a personal impression, and this, even in a very careful and observant person, is just as likely to be wrong as right. There are several other reasons that invalidate the inquiry and it may therefore be dismissed as useless for my purpose.

In 1904 A. Kowalewski[2] repeated Colgrave's enquiry on school-children, taking care to make his question less ambiguous. He asked—"Woran kannst du dich klarer und deutlicher erinnern, an Freuden oder an Leiden?" ("Can you remember joys or sorrows more clearly and distinctly?"). The results were similar to those of Colgrave, and Kowalewski tabulates them thus:

---

[1] *Amer. J. of Psychol.* x, 228–55; and *Memory*, London, 1901.

[2] *Studien zur Psychologie des Pessimismus*, Wiesbaden, 1904.

THE INFLUENCE OF FEELING ON MEMORY

BY A. WOHLGEMUTH.

I. *Previous experiments* (pp. 405–410).
II. *Observations thereon* (pp. 410–413).
III. *Own experiments and conclusions* (pp. 413–416).

I.

THE statement that unpleasant experiences are more easily forgotten than pleasant ones is often found in psycho-analytic literature. Whether this is true or not can, I believe, only be settled by experimental psychological investigation; psycho-analysis does not, and cannot, furnish a scientific proof one way or the other. Nevertheless the above statement is postulated as a fact, and I have come across the assertion that "the fact itself is beyond dispute, and has not been questioned by anyone who has seriously investigated the phenomenon, either experimentally or clinically."

Now what is the experimental evidence? I will take it, as far as it is known to me, in chronological order.

In 1898 F. W. Colgrave[1] made use of the questionnaire-method when investigating the phenomena of memory. Question 8 ran: "Do you recall pleasant or unpleasant experiences better?" Apart from the fact that this question is liable to several interpretations, the method itself in this case has little scientific value, for one can obtain from the person questioned nothing but a personal impression, and this, even in a very careful and observant person, is just as likely to be wrong as right. There are several other reasons that invalidate the inquiry and it may therefore be dismissed as useless for my purpose.

In 1904 A. Kowalewski[2] repeated Colgrave's enquiry on school-children, taking care to make his question less ambiguous. He asked—"Woran kannst du dich klarer und deutlicher erinnern, an Freuden oder an Leiden?" ("Can you remember joys or sorrows more clearly and distinctly?"). The results were similar to those of Colgrave, and Kowalewski tabulates them thus:

---

[1] *Amer. J. of Psychol.* x, 228–55; and *Memory*, London, 1901.

[2] *Studien zur Psychologie des Pessimismus*, Wiesbaden, 1904.

Table.

Pleasant and unpleasant memories of school-children, age 10–13 *years.*

	Total number of children	Number of children remembering better pleasant experiences	Number of children remembering better unpleasant experiences	Percentage in favour of pleasant experiences
Boys	124	86	38	69·4
Girls	146	99	47	67·8

I give these particulars for the sake of completeness, although they leave the question to be investigated quite unaffected.

In 1905 Kate Gordon[1] made a distinctly valuable contribution to the subject. In a first investigation coloured figures were exposed and protocols written down. Three weeks later the figures were shown again. Two tables give the results. As the number of experiments was not sufficient a further series of experiments with 50 other (black on white) pictures was performed and the results are given in two other tables. From these it is evident that *there is no essential general difference in the average numbers of the respective points remembered as regards the pleasant, unpleasant, and indifferent classes.* A second series of experiments each with nine coloured squares in square-like arrangement was performed. Forty figures were used. The results are again given in two tables and are identical with those of the preceding series.

The paper by Kate Gordon is followed by one by O. Külpe[2], in whose laboratory her investigation had taken place. Külpe discusses the bearing of the experiments on various points.

In 1908 A. Kowalewski[3], in a study of Schopenhauer and his philosophy, published a further investigation on feeling-tone and memory. The objections to his first investigation hold good no longer, but the point of view taken by the author is unwarranted and his conclusions are therefore wrong. If the results are, however, looked upon in the only way in which they can be legitimately regarded the conclusions are diametrically opposed to those at which Kowalewski arrives.

Kowalewski experimented with 105 boys of a municipal elementary school, aged from 9–15 years. On the day following a public holiday the boys were set the following task: "Write down whatever gave you *pleasure* yesterday, the pleasant things you experienced yesterday. Only what really occurred to you has to be put down. If you experienced nothing pleasant, write nothing down." It was impressed upon the boys that spelling, writing or style did not count and that no marks would be given for their answers. After ten minutes the papers were collected and on the expiration of a further five minutes a similar task was set to the boys with reference to *painful, unpleasant,* experiences. Ten days later the experiment was repeated; the boys had to write down again in a similar manner their pleasant and their unpleasant experiences on that public holiday. Kowalewski gives the boys' answers in full. The pleasant experiences put down on the first occasion he calls "primary pleasure-memories" and the unpleasant experiences "primary unpleasure-memories"; the experiences recorded on the second occasion he terms correspondingly "secondary pleasure-memories" and "secondary unpleasure-memories."

---

[1] "Über das Gedächtnis für affektiv bestimmte Eindrücke," *Arch. f. d. ges. Psychol.* 1905, IV, 437–58.

[2] "Bemerkungen zu vorstehender Abhandlung," *ibid.* 459–64.

[3] *Arthur Schopenhauer und seine Weltanschauung*, Halle a/S., 1908.

In his conclusions Kowalewski classifies the boys into "memory-optimists," "-pessimists" and "-indifferentists," according to whether the pleasant or unpleasant experiences predominated, or were equal. The two experiments thus showed:

Primary memory-optimists	61	Secondary memory-optimists	42
,, ,, pessimists	21	,, ,, pessimists	18
,, ,, indifferentists	23	,, ,, indifferentists	45

Distributing the indifferentists equally between the optimists and pessimists he gets:

Primary memory-optimists	72·5	Secondary memory-optimists	64·5
,, ,, pessimists	32·5	,, ,, pessimists	40·5

or expressed in percentages:

Primary memory-optimists	69·0
Secondary ,, ,,	61·4

Boys who are both primary and secondary memory-optimists he calls pronounced memory-optimists and boys who are both primary and secondary memory-pessimists he calls pronounced memory-pessimists. He finds that there are among his 105 boys 24 pronounced memory-optimists and only 2 pronounced memory-pessimists. By these and other similar treatments of his results Kowalewski everywhere confirms the predominance of pleasure-memory.

I need not go into any further details, for all these conclusions of Kowalewski's are valueless, as they rest upon the tacit assumption that pleasant and unpleasant experiences are equal in number. Only if this assumption were correct could we draw a conclusion from the experiments in Kowalewski's fashion as to the respective memory-values of pleasant and unpleasant experiences. But since the assumption, as I shall show presently, is wrong, the conclusion is consequently unwarranted. I will, however, defer the discussion of a different treatment of Kowalewski's figures until later.

In 1911 Wilhelm Peters[1] and in 1914 W. Peters and O. Němeček[2] published papers on the subject. The earlier work by Peters was conducted with eight subjects, 948 experiments were tried and 879 reactions were obtained. A stimulus word was called out and the subject had to react with the memory of an actual personal experience. When such an experience had been recalled the subject had to answer a number of questions put to him by the experimenter. He was asked whether the experience was, at the time of its occurrence, affectively toned or not; if it were what was the feeling-tone; whether the revival was affectively toned and how; how long ago the experience occurred; how often a similar experience had occurred and how often it had been remembered. Of the 879 revived experiences 80 per cent. experiences were affectively toned, 16 per cent. indifferent, 4 per cent. uncertain. Of the affectively toned experiences 65 per cent. were pleasantly toned, 30 per cent. unpleasantly, and 5 per cent. had a mixed feeling-tone. Old experiences were the most frequently revived. The older experiences showed a greater proportion of pleasure-tone and a smaller proportion of unpleasure-tone and neutral tone than the more recent experiences. This proves, in my estimation, nothing; for the judgment about the old experiences was the judgment at the time of the revival and this was doubtless influenced by the so-called "memory-optimism." Another conclusion at which Peters arrives is: that the unpleasantly-toned experiences that are of little significance to the experiencing individual have less chance to be remembered than the pleasantly-toned experiences of little significance. This conclusion postulates tacitly that the unpleasant experiences are of the same frequency in the individual's life as the pleasant experiences, which of course, is not the case with normal healthy persons. These are the essential points, I believe, that are of interest for my enquiry.

Recognizing that the number of subjects had been too small Peters made some mass-experiments in conjunction with Němeček. They experimented on 118 students of the age of 14–24 years with 105 stimulus words and 28 elementary school children of 10–11 years with 102 stimulus words. Peters and Němeček state that over two hours were required for these 105 experiments, which means about 1¼ minute for each experiment! The point that interests us chiefly is given in his Table 3, viz.:

---

[1] "Gefühl und Erinnerung. Beiträge zur Erinnerungsanalyse," *Psycholog. Arbeiten*, 1911, VI, 197–260.

[2] "Massenversuche über Erinnerungsassoziationen," *Fortschritte der Psychol. & ihrer Anwendungen*, 1914, II, 226–45.

	118 students	28 children	8 subjects of old experiment
	%	%	%
Pleasure	50·2	56·2	65
Unpleasure	45·6	43·8	30
Mixed tone	4·2	—	5

The affectively toned experiences which are revived are composed of a greater proportion of pleasantly-toned experiences than unpleasantly-toned ones. Thus far the conclusion is correct enough, but nothing more can be inferred. It would be fallacious to deduce from this that pleasantly-toned experiences had a greater memory-value, or that unpleasantly-toned experiences are more easily forgotten, for, as I have said already, it postulates that pleasant and unpleasant experiences are of equal frequency, which is unwarranted.

In 1918 E. C. Tolman and I. Johnson[1] published a paper. 'Pleasant,' 'unpleasant,' and 'indifferent' words were chosen, *e.g.* smooth, victory, etc.—rough, defeat, etc.—still, motion, etc. The authors state the result thus: "(1) Names of simple but unpleasant sense qualities used as stimuli, tend to lengthen association-times quite as much as do unpleasant words of deeper emotional significance. (2) Women show a tendency to be affected by unpleasant stimulus-words more than do men. (3) There is evidence in the case of women, to suggest that pleasant stimulus-words may tend to cause shorter association-times than those caused by indifferent stimulus-words. The evidence for this point, however, is slight."

Whether any feeling-tone is connected with a stimulus-word or with a reaction-word can only be ascertained by the subject's introspection, but it does not appear from this paper that such evidence was obtained. To suppose that such stimulus-words as 'smooth,' 'victory,' etc. were accompanied by a pleasant feeling-tone, or 'rough,' 'defeat,' etc., by an unpleasant one, or that 'still,' 'motion,' etc., are indifferent, is mere assumption.

In 1921 W. Whately Smith[2] attempted to measure the influence of feeling-tone upon memory by means of association experiments, employing the 'psycho-galvanic reflex,' measuring the reaction time and using Jung's reproduction test. Some of the principal conclusions the author comes to are these: Memory for words is influenced by affective tone; an intensely affectively-toned word may be better remembered or it may be more easily forgotten than a less intensely-toned word. The affective-tone as indicated by the psycho-galvanic reflex is of two kinds, the 'positive tone' which facilitates remembering and the 'negative tone' which impedes it. But positive is not identical with 'pleasant,' nor 'negative' with 'unpleasant.' The affective-tone, as shown by prolongation of reaction time, appears almost equally likely to facilitate or to impede memory .

---

[1] "A Note on Association-time and Feeling," *Amer. J. of Psychol.* 1918, XXIX, 187–95

[2] "Experiments on Memory and Affective Tone," This *Journal*, Gen Sect. 1921, XI, 236–50.

The essential drawback to this investigation is the substitution of the psycho-galvanic reflex for introspection which savours strongly of Bechterew's 'Objective Psychology' or Watson's 'Behaviorism.' The significance of the psycho-galvanic reflex is anything but settled and at the best it might only indicate, as Prideaux[1] puts it, "in the same person at the same time and under the same conditions the intensity of crude emotions as subjectively experienced." Whately Smith encountered evidently the same difficulty for he tells us that "different subjects varied considerably in the reliability of the records it was possible to obtain from them," and he has recourse to the reprehensible device of weighting his results "according to the experimenter's estimate of their reliability." We are then presented with "positive and negative affective tones" which are not identical with pleasure or unpleasure. Are these affective-tones psychic experiences? We are not told. The procedure to ascertain the "memory values" of the words learned is likewise open to most serious objection.

The work of Mayer and Orth, Wreschner, Jung, and Gött[2] need not be considered here, for it touches only the influence of the feeling-tone upon the reproduction or association-times and not the influence upon memory generally.

II.

This, I believe, is all that can be found in psychological literature upon the question of the influence of the feeling-tone upon memory.

---

[1] E. Prideaux, "Expression of Emotion in cases of mental Disorder as shown by the Psycho-galvanic Reflex," This *Journal*, Med. Sect. 1921, II, 46.

[2] Mayer and Orth, "Zur qualitativen Untersuchung d. Assoziationen," *Ztschr. f. Psychol.* 1901, XXVI; A. Wreschner, "Die Reproduktion und Assoziation von Vorstellungen," Erganzungsbände 3 and 4, *Ztschr. f. Psychol.* 1907 and 1910; C. G. Jung, "Diagnostische Assoziationstudien," *J. f. Psych. und Neurol.* Bd. 3/5, 1904/5; Th. Gött, "Assoziationsversuche an Kindern," *Ztschr. f. Kinderhlk.* I, 1905.

As we have seen, with the exception of Gordon's work, all is inconclusive; and Gordon showed that there was no difference in the memory of pleasant experiences and of unpleasant experiences. However, Offner[1] pointed out that Kowalewski's data of the second experiment[2] (described on pp. 406, 407) contained data that could be turned to some use. Offner says: "(Kowalewski) got 105 children of an elementary school to write down at the beginning of their lesson what had happened to them of a pleasant nature on the previous day, which was a public holiday, and what of an unpleasant nature. Ten days later (and it is this second test which alone gives value to the experiments) the boys were asked the same question as to what had happened to them on that holiday. And there the three tables show that with the 37 boys of the first group 1½ per cent. more of the unpleasant memories than of the pleasant of the first test were recorded; with the 52 boys of the second group even 7 per cent., and only with the smallest group of 16 boys 5 per cent. more pleasant than unpleasant memories were recorded. If Kowalewski's experiments are looked upon in this light, which as a matter of fact he does not do, one cannot arrive at his conclusion, that pleasant memories are superior to unpleasant ones, but to the very opposite, viz., that the unpleasant ones leave stronger memory-dispositions behind."

However, the matter is not quite so simple as Offner thinks. It is quite correct to take the first test as the starting point and to accept Kowalewski's figures. But his figures for the second test have to be modified, for there are quite a number of experiences recorded which cannot be found in the first test. These experiences have probably occurred on another day and have been erroneously referred to the holiday. Rectified in this way we get the following results:

First group of 37 *boys* (9–13 *years*).

		1st test	2nd test	Forgotten
Pooled	Pleasant memories	145	55	90 = 62·1 %
	Unpleasant ,,	108	46	62 = 57·4 ,,

Average percentage per child of forgotten memories	Pleasant	57·9
	Unpleasant	53·0

Second group of 52 *boys* (10–13 *years*).

		1st test	2nd test	Forgotten
Pooled	Pleasant memories	153	24	129 = 84·3 %
	Unpleasant ,,	110	27	83 = 75·5 ,,

Average percentage per child of forgotten memories	Pleasant	84·6
	Unpleasant	63 0

---

[1] Max Offner, *Das Gedächtnis*, 3te Aufl. Berlin, 1913, 85.

[2] *l.c.*, p. 161 sq.

Third group of 16 *boys* (12–15 *years*).

		1st test	2nd test	Forgotten
Pooled	Pleasant memories	58	23	35 = 60·3 %
	Unpleasant ,,	50	18	32 = 64·0 ,,

Average percentage per child of forgotten memories	Pleasant	55·0
	Unpleasant	58·4

The three groups treated together.

		1st test	Forgotten
Pooled	Pleasant memories	356	254 = 71·3 %
	Unpleasant ,,	268	177 = 66·0 ,,

Average percentage per child of forgotten memories	Pleasant	70·5 (m.v. 26·8)
	Unpleasant	58·8 (m.v. 33·2)

The results pooled, irrespective of the number of children, show that 71·3 per cent. of the pleasant memories are forgotten as against 66·0 per cent. of the unpleasant memories.

I have also treated the results in another way: I worked out the percentage for each child separately and then struck the average. This gives: pleasant memories forgotten 70·5 per cent. (m.v. 26·8); unpleasant memories forgotten 58·8 per cent. (m.v. 33·2).

I have stated above that the conclusions to which some of the investigators came were wrong because their conclusions were based upon the tacit and false assumption that pleasant and unpleasant experiences are about equal in number. This is not the case. In the normal and healthy individual the pleasant experiences by far outnumber the unpleasant ones, as personal observation will always confirm. In my experiments described below, the children recorded about 2600 pleasant experiences against about 1600 unpleasant ones.

There has been, as far as I know, no systematic attempt at the quantitative investigation of this question, with the exception of that of J. C. Flügel. The results of Flügel's work have not been published and I have to rely upon an abstract of a paper communicated to, and read before, the British Psychological Society on March 24th, 1917, entitled "A quantitative study of feeling and emotion in every-day life."

"Six subjects made records of the feelings experienced by them during the whole of their waking consciousness for a period of not less than 30 days, according to the following instructions:

> 'Suppose that 0 corresponds to a state of complete Indifference, in which you feel neither Pleasure nor Unpleasure; that +100 corresponds to the most intense Pleasure which you have ever experienced; that −100 corresponds to the most intense Unpleasure which you have ever experienced. Then, you wil

divide up this total scale of Feeling (ranging from +100 to −100) into seven grades as follows:

+3. Any state between +100 and +66.
+2. ,, ,, ,, + 66 and +33.
+1. ,, ,, ,, + 33 and 0.
0. A state of Indifference; or one in which Pleasure and Unpleasure are equally balanced (or very nearly so).
−1. Any state between 0 and − 33.
−2. ,, ,, ,, −33 and − 66.
−3. ,, ,, ,, −66 and −100.’

The subjects made entries in their records according to this plan at frequent intervals during each day, and at the end of the period of observation, the following results were calculated, indicating (in percentages) the proportion of the total period during which each intensity of Feeling was experienced.'

Distinguishing numeral of subject	+3	+2	+1	0	−1	−2	−3	$\frac{-}{+}$	$\frac{\text{Weighted}\,-}{\text{Weighted}\,+}$
1833	1·205	5·340	55·705	14·985	20·250	2·285	0·280	0·46	0·39
1076	0·995	6·635	37·815	44·240	9·695	0·625	0	0·23	0·20
3693	1·050	9·390	43·600	19·040	24·720	2·040	0·160	0·50	0·45
9621	1·510	14·160	41·725	36·050	5·550	0·950	0·050	0·12	0·12
2028	0·790	11·805	24·270	40·995	17·340	4·435	0·365	0·60	0·54
7649	2·920	17·625	33·755	17·235	20·715	6·920	0·830	0·53	0·49

The figures in the last two columns represent what might be termed the ‘algedonic’ ratio, *i.e.* the proportion of Unpleasure to Pleasure, for each subject. In calculating the weighted ratios in the last column, the values for +2 and −2 have been multiplied by 2, and those for +3 and −3 multiplied by 3.”

Flügel’s results thus confirm my contention as stated above[1].

It must, however, be borne in mind that Flügel’s “algedonic ratio” refers to duration, whilst the work done upon feeling and memory refers to numbers of experiences whose durations may vary greatly. However, I do not think that this affects the issue.

III.

My own experiments were performed on the lines of Kowalewski’s with the intention of treating the data in the same manner as I treated them, but allowing more time for them and making them on a greater number of children[2].

---

[1] p. 408.

[2] I have to thank Miss E. C. Allmond, B.Sc., and Miss F. Woodward for their kindness in performing these experiments for me.

On the morning of a day after a half-term holiday in October 1921 the children were called into the hall and there addressed by their respective head-mistresses. They were told that on returning to their class-rooms they would be given each a sheet of paper which was to be folded lengthwise. They had to write their name and age on the top and on the one side of the fold all their pleasant experiences of the preceding holiday as far as they remembered them, and on the other side of the fold all their unpleasant experiences. There would be no marks, and writing and spelling would not be of any importance. It was also stated that ample time would be allowed and that there should be no haste. A few illustrative examples were given to show the children what they were required to do. The papers were collected on completion. On a second occasion, in the one school 10 days, and in the other school 14 days, after the first test the children were without warning called again into the hall and there again addressed by their respective head-mistresses. They were told that the previous test was to be repeated and that they had to put down again all the pleasant or unpleasant experiences that they had had on the holiday in question. The papers were again collected. The two papers of each child were joined and marked together. An experience found on the second paper and not on the first was discarded.

On marking the papers it was discovered that in one of the schools, unfortunately, one illustration had been given by the teacher which fell well within the children's own range of experience, viz. that: "an extra half-hour was enjoyed in bed"; it was recorded by 75 per cent. of the children as a pleasant experience in the first test. That this illustration should be remembered on the second occasion more generally than any other experience is only to be expected, and it is indeed found in 72 per cent. of the children. The "pleasant experiences" would therefore be unduly weighted in favour of remembering, and I thought that I would be getting nearer the true state of affairs, if I discarded this answer altogether where it occurred in both tests. This has been done in the figures that follow:

First School: 403 children (age 11–16 years).

(*a*) The whole of the results pooled.

Pleasant	experiences	recorded in first paper	4180	
,,	,,	forgotten in second paper	1821	= 43·6 per cent.
Unpleasant	,,	recorded in first paper	1915	
,,	,,	forgotten in second paper	853	= 44·5 per cent.

(*b*) The percentage worked out for each child separately gave

(1) an average for pleasant experiences forgotten of 41·8 (m.v. 17·0)
and ,, ,, unpleasant ,, ,, ,, 38·1 (m.v. 23·0)

(2) a mean for pleasant experiences forgotten of 41·6
and ,, ,, unpleasant ,, ,, ,, 40·0

(*c*) The number of children

who forgot a larger percentage of pleasant experiences was 214,
,, ,, ,, ,, ,, unpleasant ,, ,, 160,
,, ,, an equal ,, ,, both ,, ,, 29.

Second School: 284 children (age 11–16 years).

(*a*) The whole of the results pooled.

Pleasant experiences recorded in first paper 2555
,, ,, forgotten in second paper 879 = 34·5 per cent.
Unpleasant ,, recorded in first paper 1576
,, ,, forgotten in second paper 553 = 35·0 per cent.

(*b*) The percentage worked out for each child separately gave

(1) an average for pleasant experiences forgotten of 33·2 (m.v. 16·4)
and ,, ,, unpleasant ,, ,, ,, 31·2 (m.v. 19·4)

(2) a mean for pleasant experiences forgotten of 33·3
and ,, ,, unpleasant ,, ,, ,, 33·3

(*c*) The number of children

who forgot a larger percentage of pleasant experiences was 131,
,, ,, ,, ,, ,, unpleasant ,, ,, 120,
,, ,, an equal ,, ,, both ,, ,, 33.

Both Schools together: 687 children (age 11–16 years).

(*a*) The whole of the results pooled.

Pleasant experiences recorded in first paper 6735
,, ,, forgotten in second paper 2700 = 40·1 per cent.
Unpleasant ,, recorded in first paper 3491
,, ,, forgotten in second paper 1406 = 39·8 per cent.

(*b*) The percentage worked out for each child separately gave

(1) an average for pleasant experiences forgotten of 38·2
and ,, ,, unpleasant ,, ,, ,, 35·3

(2) a mean for pleasant experiences forgotten of 37·5
and ,, ,, unpleasant ,, ,, ,, 33·3

(c) The number of children

who forgot a larger percentage of pleasant experiences was 34
,, ,, ,, ,, ,, unpleasant ,, ,, 28
,, ,, an equal ,, ,, both ,, ,, 6

These figures indicate sometimes a slightly greater tendency f forgetting of pleasant experiences than for the forgetting of unple ones, sometimes they indicate the reverse. This may be due in p individual differences which doubtless exist. On the whole this ten to forget seems to be slightly greater for pleasant experiences, bı mean variations being so large I consider the small excess esser due to chance and think it can be safely concluded that *there difference whatever between the two feeling-tones, pleasure and unple in their influence upon memory.*

(*Manuscript received* 1 *January* 1923.)

THE EFFECTS OF DEPRIVATION OF OXYGEN UPON MENTAL PROCESSES

By J. P. LOWSON.

(*From the Cambridge Psychological Laboratory.*)

I. *Description of tests employed* (pp. 418–421).
II. *Practice experiments* (p. 421).
III. *Observations in the chamber* (p. 421).
IV. *Subjective changes* (pp. 422–424).
V. *Objective changes* (pp. 424–431).
VI. *Conclusions* (pp. 431–434).

THE following experiments were carried out in an air-chamber, which was placed at the disposal of the Psychological Laboratory by the courtesy of Mr Barcroft of the Physiological Laboratory, Cambridge. Mr Barcroft was also kind enough to produce the atmospheres and to perform the air analyses required. The chamber in question is 9 ft. long by 6 ft. broad, with a height of 6 ft. 4 ins.; it has, therefore, a capacity of about 330 cubic ft. The sides are of glass in an iron framework. It is provided with an air lock having a capacity of 54 cubic ft., into which four persons can just squeeze. The diminished oxygen pressures were produced by replacing more or less of the contained atmosphere with nitrogen, so that while the oxygen pressure fell the total atmospheric pressure remained the same. Excess of carbonic acid gas and moisture is avoided by suitable absorbent apparatus through which the air is driven by an electric fan, which also keeps the air circulating in the chamber. Since the chamber is not absolutely air-tight, a certain leakage in and out takes place, but it will be seen from the readings given later, that this is not sufficient to replace the oxygen used up by the occupants.

The subjects who submitted themselves to experiment were five in number: namely, two naval medical officers, one army medical officer, one naval petty officer and one army non-commissioned officer. In the following record they are denoted in the above order by the letters *A*, *B*, *C*, *D*, and *E*.

I.

The tests selected were the following:

1. *Speed of Movement.* For the purpose of testing speed the Veeder tapper was used, consisting of a couple of cyclometers fixed to a board at a convenient height, each cyclometer having attached a flat metal plate, the strokes upon which are registered. The subject is given a wooden handle carrying a broad end; he holds the handle somewhat like a pen, and is instructed to tap as rapidly as possible upon the cyclometer. He is not allowed to rest his arm or wrist upon the board. He starts at a signal on one of the cyclometers and at the end of half a minute changes over to the second cyclometer where he continues for another half minute. In the following observations the differences between the first and the second half minute did not prove to be significant for the purpose of the experiments; the score is therefore stated as the number of taps performed in one minute.

2. *Accuracy of Movement.* For the purpose of testing accuracy a well-known device was used, which has been greatly improved by Prof. Bernard Muscio. In the form devised by him this consists of a series of 'targets,' ten in number, printed on sheets of paper. The sheets measure 12·5 × 25 cms., and the targets 19 mm. across. Each target consists of a central dot, 1 mm. in diameter, surrounded by nine concentric circles, the distance between the dot and the first circle, and between each circle and the next wider, being 1 mm. The targets are arranged lengthwise on the sheet in three curving rows of three, four, and three targets, respectively, and are joined by lines which indicate the track which the subject is to follow moving from left to right along the top-row of three, from right to left along the middle row of four, and from left to right along the bottom row. The subject is given a mounted needle which he holds perpendicularly. The target sheet is pinned to a cardboard fixed to the table, and the subject sits in front of it with his needle point resting on the cardboard in front of the middle of the sheet. At a signal he carries his needle across to the first target, bringing the point down as nearly as possible on the central dot; he then brings back the needle to its original position and proceeds to the second target, and so on through the series of ten in the prescribed order. After pricking the tenth target, that is, the third and last on the row nearest him, he goes to the first again, and repeats the series, and in this way his series of 'shots' may be repeated without pause a certain number of times. In this case, as in the preceding, he is not allowed to rest his

arm on the table in front of him. The rate at which the shots are made is determined by a metronome with the successive clicks of which he must time his forward and backward movements. The prick of the needle registers upon the sheet the position of each shot (the shots rarely coincide, and when they do this can usually be made out), the concentric circles give the distance in millimetres from the central dot, and the score is calculated by adding all these distances for all the shots made. Hence the greater the subject's accuracy the less the score. It is worth remark that the targets are not all on a level in difficulty, since the length of the required movement shortens from the first row of them to the third and nearest row; the sudden change in this respect from the last target back to the first is particularly apt to throw the subject out.

In the following experiments the metronome rate selected was 150 strokes per minute, that is, 75 shots were made per minute. This rate was selected after a few preliminary trials as neither too easy nor too difficult, having regard in the latter case to the comparatively small amount of time which could be devoted to practice. The subject made five rounds of the targets, giving a total of 50 shots. Where a shot missed the target altogether ten was counted into the score, a figure which does not of course represent in this case the actual distance from the centre, but it was found that at the rate mentioned complete misses were not sufficiently frequent for this method of reckoning to interfere with the results.

3. *Speed of Discrimination and Reaction.* The method used here was that of sorting cards into their respective suits, and the score was taken as the time required to sort a pack, measured in seconds. The cards were first thoroughly shuffled in the ordinary way, after which the pack was gone through card by card in order to make certain that no two cards of the same suit followed each other and that a sequence of two or three suits was not anywhere immediately repeated.

4. *Formation of Associations.* A form of substitution test was here adopted. The subject is supplied with a sheet having a star, a circle, a square, a cross, and a triangle, printed at the top and serving as a key-line. Below are rows of these figures in irregular order but so arranged that the same figure is never immediately repeated. There are ten such rows of ten figures each, 100 figures in all. The numbers 1, 2, 3, 4, and 5 are written each in one of the figures printed in the key-line, a chance order being selected for each observation but so that the same digit never appears twice in succession within the same figure, and the subject is instructed to turn the sheet over at a given signal and begin writing

in the black figures the appropriate digits, referring to the key-line for them until he knows what they are. The score is the time in seconds required to complete the sheet.

Three further tests were used which involved comparison and judgment of a simple type. The material for these tests was taken from among those which were employed by the American Army during the war in order to establish grades of intelligence among officers and men.

5. *Perception of Analogies.* The subject was supplied with a list of twenty pairs of words, the members of each pair standing to each other in a certain relation; opposite each pair is a third word followed by four others, thus:

pretty—ugly : : attract—fine repel nice draw.

The subject is required to underline the word among the last four, whose meaning stands in the same relation to that of the third word, as the meaning of the second does to the meaning of the first. The subject is instructed to guess when he does not know, and the score was taken as the time required to complete the list.

6. *Distorted Sentences.* The test consisted in the perception of the truth or falsity of a distorted sentence. The subject was given a list of twelve short sentences in each of which the order of the words was disarranged. He was required to recognise the meaning of the sentences and to express his opinion of their truth or falsity by underlining the word 'true' or the word 'false' printed opposite each of them. A specimen line runs as follows:

Every times makes mistakes person at. True. False.

Here again the score was taken as the time required to complete the list. It is perhaps worth remark that in order to recognise the meaning of such a sentence it is not always necessary to transpose the words, and the subject was not required to do this.

7. *Same-opposite Test.* This consisted in the recognition of similarity or opposition of meaning between pairs of words. The subject was given a list of twenty pairs, and was required to underline the word 'same' or the word 'opposite' printed in a line with each pair. For example:

Lax—strict. Same. Opposite.
Repress—restrain. Same. Opposite.

Similarity and opposition of meaning occur in irregular order throughout the list. As before the score was taken as the time required to complete the list.

Of these three tests 5 and 6 were used for subjects *A*, *B* and *C* (commissioned officers), while tests 5 and 7 were used with subjects *D* and *E* (non-commissioned officers).

II.

The first six sittings were occupied in practising tests 1, 2, 3, and 4; tests 5, 6, and 7 were not practised. For this purpose a sitting was held once daily for a week, after a few preliminary trials. The object was rather to accustom the subjects to the mode of performing the tests, than to eliminate the effects of practice altogether. Most of the subjects showed a certain rise in efficiency during this period, though this was not true of some of them in regard to certain tests.

III.

At the conclusion of these preliminary trials, observations were started in the chamber. For this purpose the subjects were divided into two groups, *A*, *B*, and *C* (commissioned officers) forming one group, and *D* and *E* (non-commissioned officers) the other, since the chamber was too small to accommodate all five subjects and the experimenter comfortably together. Further, at this point tests 5, 6, and 7 were introduced, tests 5 and 6 being used for Group I, and tests 5 and 7 for Group II.

The procedure was as follows: A control trial was first made under ordinary conditions; subjects and experimenter then entered the chamber; and after an hour spent there in reading, talking, or sitting quiet, the tests were again carried out within the chamber. The first test in the chamber was made under ordinary atmospheric conditions, although the subjects were under the impression, at least on entering, that the atmosphere had been altered, the object being to discover whether the change in the surroundings alone might be sufficient to produce a marked change in the performance of the tests, or whether a degree of nervousness might be produced sufficient to modify the result. Two of the subjects mentioned the slightly 'claustrophobic' effect of their first incarceration, but none of them showed any marked nervousness.

Comparing the results in these two trials, it was found that the only variations which might be considered significant were *A*'s and *C*'s performances in the substitution test (4). We may therefore conclude, so far as this observation goes, that the mere change in surroundings had no very significant effect. It was at first intended to intersperse a number of such normal tests in the chamber, but it soon became evident that the subjects could always detect the difference. For this reason, as well as for economic reasons, the idea was given up.

IV.

Before describing the results of the tests, it will be useful to give some account of the changes observed by the subjects themselves.

The percentages of oxygen employed in the trials were as follows:

Group I.

Feb. 23rd, oxygen 21 % (normal atmosphere).

Feb. 24th, oxygen 13·6 % (on entry into the chamber) to 12·8 % (just before exit from chamber); CO_2 0·57 % to 2·2 %.

Feb. 28th, oxygen 17·8 % to 16·6 %; CO_2 0·4 to 1·6 %.

March 7th, oxygen 11·1 % (taken in the middle of the period).

April 2nd, oxygen 10·1 %.

Group II.

Feb. 22nd, oxygen 21 % (normal atmosphere).

Feb. 25th, oxygen 13·6 % to 12·5 %; CO_2 1 % (at commencement).

March 2nd, oxygen 19·1 % to 17·5 %; CO_2 0·6 % to 1·1 %.

March 9th, oxygen 13·7 %.

April 4th, oxygen 9·5 % to 9·1 %; CO_2 0·5 % to 0·9 %.

It will be seen that on any day the tendency was for the oxygen to diminish somewhat in spite of a little leakage into the chamber, and for the CO_2 to increase.

In the earlier tests none of the subjects noticed any very marked change, apart from a rise in the pulse rate and a slight feeling of exhilaration. Subject *C* well expressed this feeling, when he said, that he felt as though he "had had just one drink too much" (oxygen 13·6 to 12·8 %, Feb. 24th).

When the diminution in oxygen approached 50 % of the normal pressure of oxygen, the effects noted were much more obvious.

When the oxygen was down to 11·1 %, the experimenter (myself) records that on entering the chamber he got a kind of headache in the top of his head and felt 'queer.' These uncomfortable feelings lasted for about half-an-hour, and then passed off with surprising rapidity (in about five minutes) as though some compensatory process had been suddenly completed; after this he felt quite comfortable and continued so, only he felt more inclined to talk than usual, and he thought there was some change in his general state of feeling, but could not say in what it consisted. He noticed that during the first half-hour the subjects, especially *A*, were somewhat cyanosed, and about the time that he himself felt better he noticed that they were no longer so.

Subject *C* reported afterwards that he got a feeling of constriction across the chest at first, then became tired, and lastly irritable.

Subject *A* handed in a record of his feelings which he had made from time to time in the chamber. He states, that after twenty minutes he wanted to breathe through his mouth, he felt 'weird,' his heart was thumping, and there was some confusion in his sight. He seemed especially liable to visual after-sensations; for example, the cover of his journal was green, and when he looked at it and then at the page the page seemed pink for an instant. He wondered if he was going to faint. After thirty-five minutes all these feelings passed off. His eyes felt tired but he could read. His hands and feet felt cold, and he felt slightly shivery. He took no interest in what was being said by the others who were talking; it seemed to him that *C* and the experimenter were talking rubbish, especially as they kept jumping from one subject to another. (Neither *C* nor the experimenter shares these opinions, but they are not necessarily inaccurate.) After an hour the only thing he wanted was to get on with the tests, and get out.

At 10·1 % much the same effects were produced.

B reports that he felt 'muggy' at first, and could not read. After fifteen minutes he felt nauseated, yawned constantly, noticed that his vision was slightly blurred, and that the other occupants of the chamber were cyanosed. Shortly afterwards the nausea passed off and he felt better, but he noticed that his pulse was running at 108 instead of its usual 65 per minute; he felt as though some restraint had been removed and he wanted to start talking. Voices sounded as though they were farther off than they really were.

C reports that he could not read, that he felt rather giddy at first, and after twenty minutes felt rather nauseated. After forty minutes he still felt chilly, giddy, and generally uncomfortable, and was bored and irritable.

On this occasion *A*'s feelings and the experimenter's feelings were much as previously described, except that in the case of the experimenter they never passed off completely; he noted further that he had a strong inclination to yawn, though he was not nauseated, and that voices sounded a little indistinct.

On the last occasion (Group II, oxygen 9·1 %) experimenter and subjects felt uncomfortable throughout. We were all rather nauseated, unable to read, and noticed that our vision seemed a little blurred or indistinct in some way. *D* kept on yawning. He reports that after fifty minutes he felt very giddy and 'seasick.' He controlled himself and carried out the tests, but immediately after getting out of the chamber he vomited. On leaving the chamber and breathing a normal atmosphere the experimenter noticed that for a few moments he was giddy and slightly shaky, and there were pricking sensations down the back of the thighs and in the calves and feet.

Lastly, a point of some physiological interest may be mentioned. On Feb. 28th (Group I, oxygen 17·8 to 16·6 %) a certain amount of muscular work was undertaken in the chamber by the subjects and the experimenter. After fifteen minutes in the chamber we began movements in time with a metronome beating 40 per minute. The movements were as follows: (1) rising on tiptoe, (2) full flexion of knees and hips, (3) rising again to the tiptoe position, (4) heels to ground. This of course involves lowering and raising the greater part of the body weight through the distance which full flexion and extension at the knees permit in the upright position. This exercise was continued for six minutes, and after ten minutes' rest it was repeated during another six minutes, making 120 body-lifts in all. The purpose of the exercise was to increase oxygen use. There was no apparent effect on the test performances subsequently made, and the point of interest is that we were all struck by the degree of stiffness, of the extensor muscles of the thighs particularly, which followed next day. It was painful to go down stairs, and this stiffness took two or three days to diminish markedly, and did not disappear completely for nearly a week. It is true that none of us was in training, but the effect appeared to us to be considerably in excess of what might have been expected. More than two months later, control trials were carried out under ordinary conditions and the subjects report as follows: *A* states that the repetition has produced no stiffness, but he had been riding a bicycle for the previous week; he has had no stiffness from the bicycle. *B* and *C* report mild stiffness traceable for four days after, but consider that this was not so bad as on the previous occasion. *B* says that there was not "that constant painful reminder, especially when coming downstairs" which characterized the effect of the exercise in the chamber. All three subjects agree in conclusion, that there was "no comparison" between the degree of stiffness resulting from the original exercises carried out in the chamber and those carried out subsequently under ordinary conditions, "the former being very much more marked, and lasting much longer."

V.

Passing now to the objective results, tables of the scores made by the subjects are subjoined, lettered according to the subject concerned, and remarks on the results are added. The tests were always given in the same order, namely, accuracy of movement first, then speed of movement, card test, substitution test, analogies test, and finally distorted sentences or the same-opposite test. It is to be noted that the averages given for the chamber are averages under oxygen diminution: that is, the first trial is not included.

Test 1. *Speed of Movement* (*Tapping Test*). *Score = no. of taps.*

Group I.

Date	Feb. 23	Feb. 24	Feb. 28	Mar. 7	Apr. 2	Average control tests	Average under diminished oxygen
Oxygen %	Normal air (21·0 %)	13·2 %	17·1 %	11·1 %	10·1 %		
(*A*) Control	389	—	360	354	405	377	—
Chamber	[388]	—	375	398	399	—	390
(*B*) Control	406	—	407	399	425	409	—
Chamber	[401]	—	404	413	326	—	383
(*C*) Control	431	—	375	375	377	389	—
Chamber	—	—	358	359	400	—	372

Group II.

Date	Feb. 22	Feb. 25	Mar. 2	Mar. 9	Apr. 4	Average control tests	Average under diminished oxygen
Oxygen %	Normal air (21·0 %)	13·0 %	18·3 %	13·7 %	9·3 %		
(*D*) Control	403	419	420	402	378	404	—
Chamber	[411]	397	397	392	321	—	377
(*E*) Control	472	516	471	462	473	479	—
Chamber	[494]	490	435	484	447	—	470

The *nil* result for *C* on Feb. 23rd and for *A*, *B*, and *C* on Feb. 24th was due to the tapper going wrong and ceasing to register properly.

On comparison of the scores it will be seen that no significant difference between the scores inside and outside the chamber can be established, except perhaps in the case of subject *D*, whose scores are invariably lower under oxygen diminution. It is noteworthy that *A* shows an actual improvement when in the chamber.

Test 2. *Accuracy of Movement* (*Aiming Test*). *Score = total inaccuracy in millimetres.*

Group I.

Date	Feb. 23	Feb. 24	Feb. 28	Mar. 7	Apr. 2	Average control tests	Average under diminished oxygen
Oxygen %	Normal air (21·0 %)	13·2 %	17·1 %	11·1 %	10·1 %		
(*A*) Control	67	75	64	59	49	63	—
Chamber	[57]	43	59	49	58	—	52
(*B*) Control	95	101	92	123	95	101	—
Chamber	[93]	103	105	151	122	—	120
(*C*) Control	162	121	113	109	110	123	—
Chamber	[94]	145	115	180	131	—	143

GROUP II.

Date	Feb. 22	Feb. 25	Mar. 2	Mar. 9	Apr. 4	Average control tests	Average under diminished oxygen
Oxygen %	Normal air (21·0 %)	13·0 %	18 3 %	13·7 %	9·3 %		
(*D*) Control	84	91	104	91	81	90	—
Chamber	[103]	95	85	106	209	—	124
(*E*) Control	58	70	69	79	62	67	—
Chamber	[65]	53	78	46	126	—	75

Here the results seem more definite. In Group I, *B* and *C* show a considerable drop. The average error in the chamber is, in the case of *B*, 19 % greater, and, in the case of *C*, 16 % greater than it was in the control trials. On the other hand, *A* has not been affected in the same sense. His accuracy of aim shows an increase under diminished oxygen, his average error in the chamber being 17 % less than it was in the control trials.

If we examine the individual scores, it appears that the drop in the case of *B* and *C* is mainly accounted for by their scores on March 7th and April 2nd, respectively, when the oxygen was most diminished (11·1 and 10·1 %). Though it must be acknowledged that the scores made by *C* throughout this test were erratic, still, his lowest score in the chamber (180) is 11 % worse than his lowest score outside it (162). On the other hand, *A*'s apparent improvement in the chamber holds good throughout all the trials with the exception of the last, and even here the difference can hardly be considered significant, his last score in the chamber ranking above eight of the trials outside it.

Turning to Group II, *D* and *E* both show a drop. The average error under diminished oxygen is, in the case of *D*, 42 % greater, and in the case of *E* about 12 % greater, than in the control trials. On examining the individual scores, it is seen that practically the whole of this drop is accounted for by the last trial in the chamber when the oxygen was lowest. In the case of *D* the average for the three preceding trials in the chamber is 95 as compared with 209 in the last trial, a difference of 120 %; while in the case of *E* the corresponding average is 59, representing if anything a slight increase of accuracy as compared with the scores outside the chamber, as against a score of 126, a difference of 113 %. It must be noted, however, that during this last trial the oxygen diminution resulted in nausea, a state of affairs which might be expected in any case to reduce efficiency.

Test 3. *Speed of Discrimination and Reaction* (*Card Test*).
Score in seconds.

GROUP I.

Date		Feb. 23	Feb. 24	Feb. 28	Mar. 7	Apr. 2	Average control tests	Average under diminished oxygen
Oxygen %		Normal air (21·0 %)	13·2 %	17·1 %	11·1 %	10·1 %		
(*A*)	Control	32	29	30	25	30	29	—
	Chamber	[34]	32	29	32	30	—	31
(*B*)	Control	35	35	35	33	34	34	—
	Chamber	[35]	37	37	35	41	—	37
(*C*)	Control	40	39	40	37	41	39	—
	Chamber	[35]	47	37	70	45	—	50

GROUP II.

Date		Feb. 22	Feb. 25	Mar. 2	Mar. 9	Apr. 4	Average control tests	Average under diminished oxygen
Oxygen %		Normal air (21·0 %)	13·0 %	18·3 %	13·7 %	9·3 %		
(*D*)	Control	43	29	31	34	31	34	—
	Chamber	[40]	33	32	29	55	—	37
(*E*)	Control	41	40	37	48	41	41	—
	Chamber	[49]	44	40	46	53	—	46

The results here are less marked than in the preceding test, but show indications of the effects of the greatest reductions in oxygen. In Group I, *A*, as before, shows little or no effect; his average scores in the controls and in the chamber, respectively, show a difference of two seconds, but on two occasions (and one of these the last) the scores in the chamber are just above or equal to the scores made outside, and the average score in the chamber is five seconds less than the average practice score. On the other hand, *B* shows a small but constant drop, best marked in the last trial where it amounts to an increase of seven seconds (20 %). *C* shows a marked drop in speed, amounting on the average to eleven seconds as compared with the control trials; but this difference must be discounted to some extent, since on the one occasion, March 7th, which largely accounts for this difference, he adopted an entirely new and rather absurd method of carrying out the test instead of the one he had been accustomed to use (see below); however, there can be no doubt that his mental processes were interfered with.

In Group II, both *D* and *E* show a drop, averaging in *D*'s case three seconds and in *E*'s case five seconds. Here again the biggest difference between chamber score and control score occurs in the last trial, where the drop in speed amounts to 73 % in the case of *D*, and 29 % in the case of *E*.

Test 4. *Formation of Associations* (*Substitution Test*).
Score in seconds.

GROUP I.

Date		Feb. 23	Feb. 24	Feb. 28	Mar. 7	Apr. 2	Average control tests	Average under diminished oxygen
Oxygen %		Normal air (21·0 %)	13·2 %	17·1 %	11·1 %	10·1 %		
(*A*)	Control	101	109	101	110	116	107	—
	Chamber	[115]	114	108	116	137	—	119
(*B*)	Control	125	104	117	122	112	116	—
	Chamber	[126]	116	115	108	126	—	116
(*C*)	Control	116	116	119	120	121	118	—
	Chamber	[140]	121	122	125	129	—	124

GROUP II.

Date		Feb. 22	Feb. 25	Mar. 2	Mar. 9	Apr. 4	Average control tests	Average under diminished oxygen
Oxygen %		Normal air (21·0 %)	13·0 %	18·3 %	13·7 %	9·3 %		
(*D*)	Control	126	103	113	103	107	110	—
	Chamber	[126]	105	134	111	192	—	135
(*E*)	Control	178	162	153	106	142	148	—
	Chamber	[158]	138	218	182	292	—	207

Here all the subjects with the exception of *B* show deterioration, though it may not be certain to what the deterioration is due. In Group I, *A* and *C* show a drop in speed from entry into the chamber, irrespective (so far as the figures go) of the atmosphere present. On the first trial in the chamber when the atmosphere was normal *C* showed a very marked drop, amounting to 20 % of the corresponding control; *A* showed a similar drop of nearly 14 %; *B* remained practically unaffected; *C* complained of the noise of the motor which drove the fan in the chamber, and it is not impossible that the diminution in speed on this first occasion was partly referable to this distraction. The motor was afterwards stopped during the tests.

On the following occasions, when the oxygen was diminished, *A* and *C* show always a loss of speed in the chamber, which tends towards increase with increased diminution of oxygen. In the final trial *A* shows a marked drop: his score in the chamber shows an increase of 19 % over the corresponding control and is lower than any previous score of his in the whole of his test 4 series.

In Group II, neither *D* nor *E* show any deterioration in the first trial in the chamber; thereafter both show a drop in speed, amounting on the average, as compared with the average control score, to about 22 % in the case of *D*, and 40 % in the case of *E*. If we look at the individual scores, it is apparent that in the case of *D* the bulk of the drop occurred on the last occasion; excluding the last trial the average fall in speed for the three previous trials is about 10 %. Besides the loss in speed *D* showed on the last occasion a loss in accuracy: eight out of a hundred figures were wrongly marked, whereas there were no errors in his control trials, and only one other error in the trials in the chamber (on Feb. 25th).

In *E*'s case there was a marked drop in speed in the last three chamber trials, but especially in the final one where it amounted to a difference of 106 %. There was no *parallel* drop in accuracy, though there was marked inaccuracy for some of the scores in the chamber as compared with those made outside. There were no errors in *E*'s controls, while there were three in the chamber on Feb. 22nd (oxygen normal), seven on March 2nd (oxygen 18·3 %), and one on April 4th (oxygen 9·3 %).

Test 5. *Analogies Test. Score in seconds.*

GROUP I. Scores.

Date		Feb. 23	Feb. 24	Mar. 7	Apr. 2	Average control tests	Average under diminished oxygen
Oxygen %		Normal air (21·0 %)	13·2 %	11·1 %	10·1 %		
(*A*)	Control	72	115	93	91	93	—
	Chamber	[121]	90	89	90	—	90
(*B*)	Control	72	106	149	121	112	—
	Chamber	[104]	90	114	103	—	102
(*C*)	Control	80	112	96	87	94	—
	Chamber	[137]	85	109	97	—	97

GROUP I. Number of errors.

		Feb. 23	Feb. 24	Mar. 7	Apr. 2	Average control tests	Average under diminished oxygen
(*A*)	Control	0	1	1	0	0·5	—
	Chamber	[4]	3	2	2	—	2·3
(*B*)	Control	6	5	4	2	4·25	—
	Chamber	[4]	3	4	1	—	2·7
(*C*)	Control	1	0	1	0	0·5	—
	Chamber	[4]	1	1	0	—	0·7

GROUP II.

Date		Feb. 22	Feb. 25	Mar. 2	Mar. 9	Apr. 4	Average control tests	Average under diminished oxygen
Oxygen %		Normal air (21·0 %)	13·0 %	18·3 %	13·7 %	9·3 %		
(*D*)	Control	130	138	83	100	95	109	—
	Chamber	[155]	153	130	97	156	—	134
(*E*)	Control	181	122	103	94	84	117	—
	Chamber	[125]	118	94	81	91	—	96

GROUP II. Number of errors.

		Feb. 22	Feb. 25	Mar. 2	Mar. 9	Apr. 4	Average control tests	Average under diminished oxygen
(*D*)	Control	0	3	1	1	0	1·0	—
	Chamber	[4]	3	4	3	8	—	4·5
(*E*)	Control	4	4	3	1	2	2·8	—
	Chamber	[5]	3	3	1	1	—	2·0

In this test wrong answers were not infrequent; the additional tables given above show the number of errors made.

It is doubtful whether any conclusion relative to oxygen diminution can be drawn from these results. The fact is that it is difficult to make sure that two or more series of analogies are really comparable with each other in all important respects; one list, for example, may contain by chance a much larger number of words, or rather ideas, with which the subject is familiar than another does, although the lists appear on the surface to be equal in difficulty. Thus in the control observations, all the subjects in Group I seem to have found the first list easier than the last (to judge by their scores), while the subjects in Group II seem to have found the first list difficult and the last list comparatively easy.

Test 6. *Distorted Sentences. Score in seconds.*

GROUP I only. Scores.

Date		Feb. 23	Feb. 24	Mar. 7	Apr. 2	Average control tests	Average under diminished oxygen
Oxygen %		Normal air (21·0 %)	13·2 %	11·1 %	10 1 %		
(*A*)	Control	52	57	73	58	60	—
	Chamber	[80]	60	69	65	—	64·6
(*B*)	Control	95	61	63	70	72·2	—
	Chamber	[90]	78	79	84	—	77
(*C*)	Control	65	48	62	51	56·5	—
	Chamber	[71]	52	53	55	—	53·3
		Number of Errors.					
(*A*)	Control	3	0	0	0	0·75	—
	Chamber	[1]	0	0	0	—	0
(*B*)	Control	4	3	3	2	3·00	—
	Chamber	[0]	0	1	3	—	1 33
(*C*)	Control	1	0	0	0	0·25	—
	Chamber	[0]	0	0	0	—	0

Here again the observations appear insufficient to justify any conclusion. As regards *A* and *C*, whose scores have a certain rough parallelism with each other, there is no significant difference between scores in the chamber and scores outside it, nor is there any difference in accuracy under diminished oxygen. In *B*'s case the score under diminished oxygen is regularly lower, that is, the time taken is greater, and his accuracy in the chamber appears to diminish with diminished oxygen; but his accuracy in the chamber is on the average considerably greater than his accuracy outside it, and even at its lowest point it does not fall below this outside average. We cannot, then, attach importance to this last point. However, it is perhaps worth noting that *B*, who alone shows a definite drop in speed under diminished oxygen, experienced more difficulty with this test both inside and outside the chamber than *A* and *C* did.

Test 7. *Same-Opposite Test. Score in seconds.*

GROUP II only. Scores.

Date		Feb. 22 Normal air	Feb. 25	Mar. 2	Mar. 9	Apr. 4	Average control tests	Average under diminished oxygen
Oxygen %		(21·0 %)	13·0 %	18·3 %	13·7 %	9·3 %		
(*D*)	Control	81	74	59	60	60	67	—
	Chamber	[88]	78	69	64	62	—	68
(*E*)	Control	81	57	59	59	52	62	—
	Chamber	[60]	62	62	49	55	—	57
		Number of Errors.						
(*D*)	Control	4	4	0	3	3	2·80	—
	Chamber	[1]	1	5	3	6	—	3·75
(*E*)	Control	0	4	3	2	2	2·20	—
	Chamber	[4]	1	4	3	4	—	3·00

Here *D* shows no significant effect upon speed, though he shows an average drop in accuracy amounting to nearly 34 %. *E* shows no effect upon average speed, if we exclude, as we may, the control trials on Feb. 22nd, and, similarly, his average accuracy is only 9 % less under diminished oxygen as compared with the average accuracy of the control trials.

VI.

What conclusions are we justified in drawing from these observations relative to the effect of diminished oxygen upon mental process? In this connexion there are several points that have to be noted. In the first place, it is to be noted that we are not primarily concerned here with the physiological conditions under which such effects are produced. These would require separate study. Secondly, it has to be remembered that the number of observations for each test was small, and that the tests employed were all of a very simple character: speed of movement, accuracy of movement, speed of discrimination and reaction in a relatively very simple task, speed of formation of simple associations, and three simple tests of association and judgment.

Under these limitations, then, we can conclude—first, that of all the performances speed of movement (where the movement is a familiar one) is least likely to be affected; secondly, that accuracy of movement is likely to be affected as soon as the diminution of oxygen pressure approaches 50 % of the normal, and that the drop may take place rather suddenly (subjects *D* and *E*); thirdly, that 'discrimination time' is less likely to be markedly affected (where the discriminations required are habitual), but that where this does occur the drop seems to occur rather suddenly (subject *D*); and fourthly, that the speed of formation of associations tends to be definitely affected when the oxygen pressure is near 50 % of the normal (subject *A*, for example, who showed no deterioration in the preceding tests, showed a deterioration here). With regard to the three remaining tests not much can be said, except that on the whole they seemed to show a resistance to oxygen deprivation.

It is of interest to compare these results with those obtained by Bagby[1] with a re-breathing apparatus. In his experiments, which were conducted for the purpose of selecting men for the American Air Force, the subjects were progressively asphyxiated until collapse was imminent.

Bagby found that as regards speed of movement (tapping test) the score remained fairly constant until collapse approached, while as regards aiming (using a stylus and graded openings in a metal plate) there was a constant but irregular decrease in steadiness during the preliminary stages. His most interesting remark refers to a restriction of the field of attention which he found to occur, but to this point I shall return.

Hitherto I have been dealing with the quantitative results, and it is evident that so far as the observations go these are not of a very striking character. One might perhaps conclude from them that an aviator, flying up to 17,000 or 18,000 feet, would be able to make the habitual judgments and carry out the accustomed movements which the management of an aeroplane requires with sufficient correctness to secure his safety, so long as no emergency arose; and experience seems to have borne this out. But I should like now to make some remarks on the observational and introspective side. In the course of the experiments mental effects were produced which are not evident in the test records, and about these I wish to say something.

The first of these effects is the feeling of slight exhilaration already referred to, which occurred in the earlier trials and was well compared by one of the subjects to the effect of a slight overdose of alcohol. It would appear that there was a diminution of the inhibition which certain higher mental systems normally exert: with the exception of *A*, we were all inclined to be more talkative in the chamber than outside it.

Secondly, we experienced a change of mood. The irritability shown in *A*'s notes, already cited, was also obvious in *C*. This was noteworthy in his case, since outside the chamber *C*—if he will allow me to say so—was not only courteous but gave the impression that this was natural to him. In the chamber, when the oxygen was diminished by nearly 50 %, he was inclined to be 'snappy.' It seems reasonable to refer this irritability in part to the diminution of inhibition already mentioned, combined perhaps with the moderate degree of physical discomfort to which we were all subjected.

Lastly, it seems quite certain that thinking did not go forward in the chamber with diminished oxygen under quite the same conditions as outside it. There was a difference, though it was difficult to say exactly in what it consisted. With the oxygen at 11·1 %, that is to say diminished by nearly half, all the subjects experienced difficulty in reading, and it seems safe to say that no one had the same 'grip' of what he was doing as he had under normal conditions. As regards the experimenter, it may be noted that he took half-an-hour instead of the usual fifteen minutes to put the subjects through the tests, and subject *C* noted that with the oxygen at 10·1 % the experimenter showed some slowing of speech and distinct slowness of all his reactions. (I did not notice this myself, but the time occupied in testing bore it out.) Similarly, subjects *B* and *C* were obviously affected, not so much with regard to the speed with which they performed the tests, as with regard to their manner of performing them. *B* showed this by becoming somewhat muddled in the aiming test (2) on March 7th (see his score), tending to lose the rhythm. *C* showed the same slight confusion in a still more striking way. On March 7th, instead of turning the faces of the cards towards him (test 3) as he had always previously done, he held them in the position for dealing with their backs up, so that he was obliged to turn each card over to see what it was before he put it down. No remark was made, and he carried out the whole of the test in this manner.

---

[1] E. Bagby, "Psychopathology under Low Oxygen Tension," *U.S.A. Air Medical Service*, 1920, I, 39–43.

All the subjects noticed a difficulty in reading when the oxygen diminution approached 50 %. They describe or account for this difficulty in different ways. The subjects in Group I (commissioned officers), say, that they were "unable to concentrate"; while the subjects in Group II (non-commissioned officers) seemed to attribute the difficulty to "headache between the eyes," or "soreness at the back of the eyes." I spent an hour in the chamber by myself endeavouring to find out in what the difficulty lay, but this was not easy to determine. I took two books with me—one a dull and somewhat difficult technical book, and the other a book in which I was very much interested—but I found I could read neither with comfort. The difficulty was not a gross one: it did not lie, for example, in understanding single sentences; the meaning of a sentence was always perfectly clear. My impression was that it lay rather in getting the meanings of different sentences to join up together. Thus one could read a page and understand every sentence in it, but at the end one had not a proper grasp of the contents of the page as a whole, though of course one could say what the page was about. My impression, then, was that the functions which had gone wrong were those which were concerned not directly with the focus of attention, but rather with what goes on outside this focus, though in relation with it. If one might speculate a little further, it might be suggested that what was wanting was probably the formation of a cumulative disposition in relation to what was read. We know that such cumulative dispositions are of very great importance in our ordinary thinking. In reading a book, for example, what happens is not merely that a 'stream' of ideas and accompanying affects and 'impulses' is excited in our minds, but that a more or less integrated pattern of meaning is gradually developed and goes on growing, as we get farther and farther into the book. It is part of the art of the writer to arrange and express his material in such a way as to facilitate this development in the reader's mind. The point I wish to stress is that the integrated dispositional pattern, which underlies our grasp of the meaning of a book when we have finished it, is something which is dynamic and not static, so long as the meaning is present to us in the sense of influencing our thinking, whether consciously or not; it must, then, correspond on the cerebral side to a very complex pattern of neural activity. It might be suggested, then, that the difficulty under discussion may have been due to a failure of this function, and in this way we should connect it with the effect already mentioned on the formation of new associations; for the integration of fresh mental material must have this formation as one of its fundamental conditions, whatever its other conditions may be. It is of some interest to compare with this suggestion the conclusions arrived at by Bagby in regard to the effect produced on attention by oxygen deprivation, to which I have already referred. I did not see his paper until after my experiments were completed and my conclusions were drawn. He says, "Low oxygen tension tends to bring about a reduction in the ability to carry on a number of discrete tasks simultaneously through rapid shifting of the attention. This restriction of attention operates in such a way as to lower the subject's distractibility. Hence the attention directed to a simple, relatively mechanical, feature of a complex task is superior to the attention given to that feature under normal air conditions.... In the final stage of asphyxiation the subject is unable to concentrate in the normal way and instructions have to be given insistently and in a loud voice." The effect described is, Bagby says, a restriction of the field of attention, and it will be seen that though the point to which he directs our attention is not quite the same as that just discussed, yet the two are not in contradiction, but rather supplement, and so far confirm, each other. Given that the formation of associations is interfered with and that the mobility of attention is somehow restricted—in part, perhaps, by this very fact—it would not be surprising if cumulative patterns failed, while habitual associative connexions remained intact or nearly so; for the trend of these observations is on the whole to show, that established associations resist oxygen deprivation in a high degree.

(*Manuscript received* 9 *March*, 1922.)

A PRELIMINARY NOTE ON A NEW METHOD OF DETERMINING THE PHASE EFFECT IN THE LOCALIZATION OF SOUND[1]

BY H. BANISTER.

(*From the Cambridge Psychological Laboratory.*)

THE recent researches of Appleton[2] show (*a*) that, when the difference of frequencies of two interacting triodes is less than a certain amount, automatic synchronization takes place; (*b*) that the two oscillators vibrate with the same frequency during the 'silent period'; and (*c*) that their relative phases change by π during the period of synchronization.

Since the frequencies can be varied throughout the whole range of audible sounds, the use of oscillators as a means of investigating the Phase Effect suggests itself. Such an investigation is being carried out in this laboratory.

There are two settings, *vide* Figs. 1 and 2. With the first, in which there is direct interaction between the two sets, the difference of phase produced as the two frequencies are moved through the region of synchronization is from $0 \rightarrow \pi/2 \rightarrow \pi$. With the second setting, in which the interaction is between the one set and the Grid circuit of the other, the phase difference varies from $-\pi/2 \rightarrow 0 \rightarrow +\pi/2$.

Two problems are at present being studied, viz.:

(*a*) the effect of changing the intensity of the sound at one ear when there is a constant difference of phase between the two sounds, both of the same frequency, at the two ears; and

(*b*) the relation between frequency and phase effect.

Experiments show that, as observed by Wilson and Myers[3] and by Rayleigh[4], a difference of phase between the sounds at the two ears may cause a displacement of the point of localization of the sound, even though the intensities at the two ears are not the same and are such as to counteract the phase effect. But there is more than this. Starting with equal intensities at the two ears, and a phase difference of $\pi/2$ between the two notes, a certain displacement to one side is observed. If now the intensity of the sound at the other ear is increased, by adjusting the shunt to that telephone, the position of the localized sound does not change. The sound remains stationary and eventually, after the intensity has been increased sufficiently, a second sound is heard in the other ear. The two sounds then remain distinct, unless the intensity of the one is such as to drown the other. If, when the two sounds can be heard, the phases at the two ears are made the same, the two sounds become one, and are localized on the side of greater intensity: they become two again when the phases are again made to differ by $\pi/2$. This would seem to be evidence in favour of the view that the phase effect cannot be explained in terms of intensity.

---

[1] I wish to acknowledge my indebtedness to Dr E. V. Appleton for his ever-willing advice and assistance with apparatus.

[2] "The Automatic Synchronization of Triode Oscillators," *Camb. Phil. Soc.* XXI, 231.

[3] This *Journal*, II, 1908, 363.

[4] *Phil. Mag.* XIII, 1907, 214.

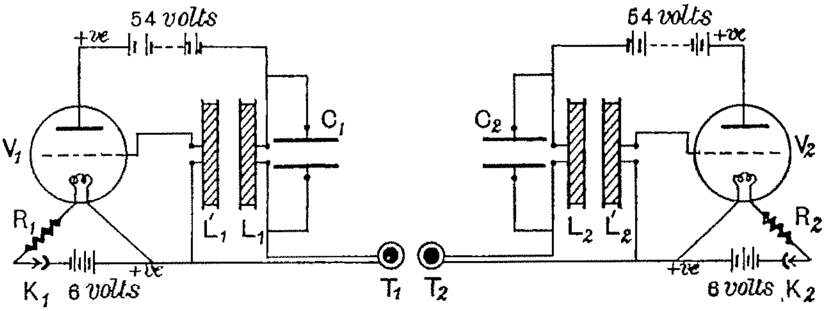


Fig. 1.

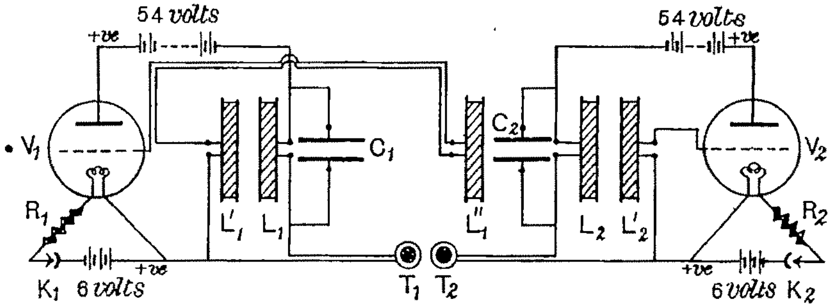


Fig. 2.

V = Marconi 'R' value. L = Inductance. C = Condenser.
R = Variable Resistance. K = Key. T = Telephone.

It is of interest to note, also, that, in the preliminary experiments, the phase effect has been observed with a frequency of about 1200 d.v. per second, agreeing with Stewart's[1] estimate of the probable limit being from 1000 to 1500 d.v. per second.

---

[1] *Phys. Rev.* N.S. 1920, xv, 482.

(*Manuscript received* 27 *February*, 1923.)

PUBLICATIONS RECENTLY RECEIVED

Beyond the Pleasure Principle. By Dr SIGM. FREUD. Translated from the second German edition by C. J. M. HUBBACK. London and Vienna: The International Psycho-Analytical Press. Pp. 90. 6*s*. net.

The first three sections are devoted to an attempt to give precision to the 'pleasure principle,' and to the demonstration that "it is not strictly correct to speak of a supremacy of the pleasure principle over the course of psychic processes." The main evidence is found in the existence of "a repetition-compulsion" which "seems to us more primitive, more elementary, more instinctive than the pleasure principle which is displaced by it." In the following sections Freud is admittedly speculating—indulging in "the exploitation of an idea out of curiosity to see whither it will lead." It leads to the endeavour to envisage life as a circuitous and often postponed quest for death. Arguing from the compulsion to repetition it would seem that an instinct is "a tendency in living organic matter impelling it towards the reinstatement of an earlier condition." The ego instincts offer no great obstacle in Freud's hands, for they may represent the refusal of particular organizations of living matter to attain the peace of the return to the inorganic by any other path than the one peculiar to it. It is the sexual instincts which provide the difficulty, for they represent life instincts, and seem to be as primitive as the death instincts. Admitting that he is "working here at an equation with two unknown quantities," Freud nevertheless feels justified in basing a final "daring assumption" on the myth, incorporated in Plato's Symposium, and also in the Brihad-Aranyaka Upanishad, that sex represents an original splitting into two of a primordial unity, and that sex attraction is the effort of the divided to reunite.

Remembering and Forgetting. By Professor T. H. PEAR. London: Methuen & Co. 1922. Pp. xii + 242. 7*s*. 6*d*. net.

This is, in the main, a book on imagery. The author is led away from the specific problem with which he starts, and a very large part of the book is taken up with an admirably clear account of the Freudian theory of dreams, with a chapter on a criticism of Rivers's views. We remember—in the restricted sense of the word adopted by Professor Pear—by means of imagery, and images are the vehicles of meaning.

Forgetting is dealt with in a chapter towards the end, containing an interesting classification of things forgotten, one of the classes being that containing our "dead selves." This is typical of so much in the book. It is entertaining and suggestive but requires far more careful analysis and attention than it receives.

An appendix contains an interesting account of some investigations into synaesthesia and number forms, showing how varied can be the imagery in connection with the same notions.

A particularly valuable contribution which the book makes is the account of psychoanalytic views. Professor Pear is unprejudiced and sympathetic,—qualities which are seldom found in critics of the Freudian Theory.

Intelligenz und Begabungsprüfungen. Von Dr J. SUTER. Zurich: Rascher & Cie. 1922. S. 180.

This is an interesting monograph. The author begins with a fairly comprehensive study of the present position in regard to the investigation of human endowments, and then proceeds to a careful discussion of the method of intelligence tests considered from the standpoint of experimental psychology. He devotes the greater part of this section to a study of tests of thinking, particularly of "logical thinking," but pays some attention also to tests of imagination and memory. He next turns to an account of some of the psychological methods by which human capacities may be tested. Here he deals rather briefly with acuity of vision, with the delicacy of the tactual perception of difference, with keenness of perception of movement, and with speed of movement. He describes a number of simple but interesting pieces of apparatus which can be used for the above tests. The final section dealing with the problem of vocational tests is very summary.

Studies in Psychoanalysis. By Dr C. BAUDOUIN. Translated by E. and C. PAUL. London: Allen and Unwin, Ltd. 1922. Pp. 352. 12*s.* 6*d.* net.

Baudouin says—"My aim has been to produce a record of my own work which might interest experts, not to write a popular treatise. On the other hand I was writing for French readers, most of whom are beginners in this subject." Later he adds—"I am sure that a discussion of the relationships between psychoanalysis and ordinary psychology is essential, and I believe that it will throw light on many points that are still obscure. In the field of theory I have endeavoured to show the ties between the psychology which has now become classical, that of Ribot, James, Bergson, and Claparède, to say nothing of the almost forgotten Jouffroy."

These two aims are so divergent and so incompatible, in a work of one volume, that it is not surprising to find that the writer fails to achieve any one of them. One-third of the book is devoted to a theoretical exposition of psychoanalysis. The remaining two-thirds provide the reader with 27 cases which the author has treated by the mixed method of psychoanalysis and suggestion. The bare narration of the cases does not give the details which might have made them useful. The glossary at the end will prove useful to readers not familiar with psychological terms. In French the book may have served a useful purpose; its translation into English was unnecessary.

Das Psychologische Profil: Eine Anleitung zur Erforschung der psychischen Functionen des normalen und des anormalen Kindes. Von Dr KARL BARTSCH. Halle an der Saale: Carl Marhold. 1922. Pp. 62. M. 25.

A brief foreword points out the importance of determining the exact nature of the normality or abnormality of school-children. Particularly in the case of subnormal children the teacher must endeavour to heal and restore as well as to instruct. Three sections follow, the first dealing with the tests proposed by de Sanctis, the second with the Rossolimo revision of these, and the third with further elaborations introduced by the author. The test material is given, with instructions how to use it. Finally the author shows how to arrive at "the profile" itself—the psychological summary and comparative evaluation of a child's capacities. Many examples are given.

The Elements of Scientific Psychology. By Professor KNIGHT DUNLAP. Baltimore and London: Henry Kimpton. 1922. Pp. 368. 18*s.* net.

This is a new and very comprehensive psychological text-book. Dr Dunlap points out that psychology is now being called upon to help to solve the problems of daily life, and to contribute its share towards the development of physical science, education, industry, and social life. It is, he holds, not the old psychology, with its introspective method, and its "peculiar world of psychic objects," which is able to help in "any problem of real life"; but the "scientific psychology whose subject matter is the world of real objects and real activities and whose methods are those of real science." His text-book is intended to put forward the aims, claims and methods of this scientific psychology.

The book attempts to cover a very large amount of ground. It begins with the various modes of sensory reaction, and discusses their experimental investigation in an interesting manner. Then comes a summary and rather inadequate chapter on "Thought and Thought Content," which deals with imagination, memory, anticipation and similar mental responses. A study of the bodily mechanism leads to a brief consideration of "Reaction and Consciousness," and this is followed by a more detailed survey of instinct and habit. There are two appendices, of which the first deals with mental disease, and the second presents a short, but on the whole, a well selected bibliography.

In this country Dr Dunlap's book will at once challenge comparison with Myers's *Text Book of Experimental Psychology*. On the whole, excellent as it undoubtedly is in many respects, it does not come particularly well out of the comparison. It contains more, but it is distinctly less concise in its expression, and its material is less well selected. Sometimes as, perhaps, in the chapter on space perception, material is included which is as yet more a matter for research than for text-books. Moreover it is not by any means clear that Dr Dunlap has anything very new to say, and sometimes, as in the Introduction, he gets perilously near to the type of philosophic psychology which, in the preface, he condemns.

When all is said, however, the book remains in many ways a very good one indeed. It is interesting to read, and is clearly expressed. The student who is by it initiated into the study of scientific psychology will be fairly certain to wish to carry his investigations further.

Group Tests of Intelligence. By Dr P. B. BALLARD. London: Hodder & Stoughton, Ltd. 1922. Pp. x + 252. 6*s*. net.

This book gives a clear account of the genesis, nature and use of Group Intelligence Tests. With its assistance a teacher who has had no previous psychological training can learn to apply the tests and to interpret the results.

Such elementary statistical methods as are necessary are described in two excellent chapters, which rob this part of the work of the terror it usually inspires in the uninitiated; while the chapter on school organization may be read with profit by all interested in education.

Religion und Suggestion. Von Dr ALEXANDER BEYER. Halle an der Saale: Carl Marhold. 1922. Pp. 122.

A careful study of some of the outstanding phenomena of suggestion leads to the conclusion that its success depends upon a type of belief achieved through concentration of thought (*Gedankenkonzentration*) and that as such, suggestion has nothing to do with religion.

Suggestion and Common Sense. By Dr R. ALLEN BENNETT. Bristol: John Wright & Sons, Ltd. 1922. Pp. 105. 6*s*. net.

Crime: its Cause and Treatment. By CLARENCE DARROW. London: Harrap & Co., Ltd. 1922. Pp. x + 292. 10*s*. 6*d*. net.

L'Enigme du Monde et sa Solution. Selon CHARLES SECRÉTAN. Paris: Alcan. 1922. Pp. 235. 12 fr. net.

The Evolution of Knowledge. By GEORGE SHANN. London: Longmans, Green & Co. 1922. Pp. 100. 4*s*. 6*d*. net.

Einführung in die Ethik auf Grundlage der Erfahrung. Von Professor G. HEYMANS. Zweite Auflage. Leipsic: Barth. 1922. S. v + 323. M. 90.

NOTES ON RECENT PERIODICALS

The Psychological Review, Vol. XXIX.

No. 3, May 1922.

The Ontogenetic Significance of Instinct, Habit, and Intelligence. (James L. Mursell.)

The object of this discussion is to give a "very schematic but accurate and clear-cut picture of the mental life of the individual." The problem of intelligence is dealt with far less adequately and suggestively than that of instinct and the relation of habit to instinct.

Practical Logic and Color Theories. (Christine Ladd-Franklin.)

A vigorous defence of the author's own theory of colour, and attack upon other theories, under the quaint guise of an exercise in 'practical' logic, that is, a logic which includes the search for 'adequate' premises, and is not merely concerned with the validity of structures of premises. The chief value of the paper is found in its replies to recent criticisms of the Ladd-Franklin colour theory.

The Significance of Psychical Monism for Psychological Theory. (Leonard Thompson Troland.)

Synaesthesia, A Form of Perception. (Raymond H. Wheeler and Thomas D. Cutsforth.)

A study of synaesthesia in one of the authors, a blind subject, in whom coloured hearing and allied synaesthetic phenomena of sensory origin are normal methods of perceiving. It is concluded that synaesthesia is in general a cognitive process, a phenomenon of meaning, and that the secondary image is essential. By means of its functioning, sensation develops to perception. The sensory component and the image component are necessary but their order of appearance may vary.

A Comparison of Mental Abilities of Mixed and Full Blood Indians on a Basis of Education. (Thomas R. Garth.)

"Under conditions of uncontrolled social status but controlled school training," the 'mixed blood' Indians excel the 'full bloods' by about 11 per cent. in performing tests of higher mental processes, increased education producing an increase in score for both groups. The average score of the mixed bloods is always above that of the full bloods, but the best scores are about equally divided between the two groups.

Discussion: Induction and Radical Psychology. (Victor S. Yarros.)

No. 4, July 1922.

Psychology and Psychiatry. (Shepherd Ivory Franz.)

A general plea for the mutual helpfulness of psychology and psychiatry, in which many of the psychiatrist's criticisms of psychological science are thoroughly met.

What is Psychology? (B. H. Bode.)

On the author's view, Watsonian behaviourism was an inevitable reaction to the psychology of mental states, but suffers under the same evil heritage of a metaphysical dualism as the psychology which it scorns. It retains the same conception of bodily behaviour, viz.: that all bodily behaviour can be described in terms of reflexes. For the author, the starting point of psychology proper is the total experiential situation, in which the outstanding fact is that the 'stimulus' is constantly changing in a way having no significance for the physicist but of the utmost importance for the living individual, and therefore for the psychologist.

Suggestions for a Compromise of Existing Controversies in Psychology. (W. P. Pillsbury.)

"My answer then to the question asked in the title of the paper is a negative. The conflicting tendencies are not to be harmonised. They are differences that have existed since the beginnings of thought. They are not amenable to investigation, because they are not matters of fact but of attitude. The conflicting attitudes are rooted in something like instinct or in the nature of things themselves and of the thinking beings that have been evolved because of the nature of things."

An Analysis of Psychological Language Data. (J. R. Kantor.)

A distinction is made between anthropological, philological and psychological language data, the two former often being simply stimuli in the psychological situation. Language responses are inherently indirect or referential adaptations to stimuli, and should not be looked upon as mere outward manifestations of mental states. The author refuses to identify language and thought, holding them to be different types of adaptive response which operate very differently.

A New Approach to the Study of Genius. (Lewis M. Terman.)

A preliminary account of an extensive investigation into the juvenile history, family conditions, school standing and detailed psychological characteristics of 1000 of the brightest children in California, having a Binet I.Q. above 140, with a view to the study of genius in the making. The author finds that many of his former views as to the later destiny of precocious children are not justified. It is hoped to make a report of salient findings this year, but the detailed treatment of data may occupy several years and the follow-up work an additional decade or two. The author rightly suggests that the value of the data will be greater twenty or thirty years hence than at present. The first results given here tentatively are of the greatest interest, and the whole investigation is one of first-rate importance for all problems of ontogenetics, not alone that of intelligence.

A Practical Definition of Character. (Raymond O. Filter.)

A discussion of various explicit or implied definitions of character, leading to the view that the modern tendency is to rule out of the psychological concept of character, not only intellectual values, but also aesthetic and moral values. "The emphasis is upon the force of activity rather than upon its direction, upon the quality of behaviour in terms of strength, persistence, readiness, rapidity, etc., rather than upon its value as right or wrong, good or bad, wise or foolish, etc."

Discussions: (1) *Conscious Analysis, Introspection, and Behaviourism.* (F. A. C. Perrin.)
(2) *The Term 'Practice.'* (E. B. Titchener.)

No. 5, September 1922.

Behaviour and the Central Nervous System. (Albert P. Weiss.)

This is in large part a reply to Warren's paper on "Psychology and the Central Nervous System" (*Psychological Review*, XXVIII. 249–69). For the writer, behaviourism is the science that studies the origin and development of those bodily movements of the organism which establish its social status. His emphasis throughout is on what he calls the "individual-social aspect," whether of the stimulating situation or of the overt reaction. Where Warren assumes a specific central factor to account for the responses which are physically incommensurate with each other (*e.g.* writing as compared with vocalising), but which yet have the same 'significance,' Weiss holds that the 'equivalence' of such responses is due to the fact that they are socially equivalent stimuli (signals) for other organisms having the same social status. And he does not believe that the behaviourist and the introspectionist can ever 'get together.' "The behaviourist is more concerned with the movements of *other organisms* than he is with his own."

How Are Our Instincts Acquired? (Zing Yang Kuo.)

A further statement of the author's view that 'instincts' should be given up in psychology, with a tentative analysis of the process of the acquisition of habits or 'instincts.' "In the newly born organism, man and animal alike, there are found a number of psychologically unorganized and chaotic bodily movements—the units of reaction. These are later integrated into various reaction systems. Two types of integration can be distinguished, namely, simultaneous integration and temporal integration, the former being the organization of the body-parts into unitary responses;

the latter, the arrangement of individual acts into sequential order." And these latter *are* the 'instincts.' Further, a *behaviour-set* is postulated to explain purposive behaviour in purely mechanistic and objective terms.

Intelligence and Learning. (Joseph Peterson.)

A general discussion of the relation of intelligence and learning, with some valuable remarks on the specific aspects of this problem as it enters into intelligence testing.

The Organismal Point of View in the Study of Motor and Mental Learning. (J. A. Melrose.)

By the 'organismal point of view' the author means that the proper study of the psychologist is not the movements of parts (glands, muscles, etc.), but the "patterns of the adaptations which the *organism as a whole* makes to its environment and the *controllable factors* which so affect the environment and the organism as to modify learning." He distinguishes various types of learning in sub-human animals, as an approach to the "distinctive problem of human psychology—the problem of 'thinking,' in terms of the 'organismal' theory and method." And in order to get at the specific element in human learning, we must "look for the *pattern of the most characteristically human behaviour*....Such behaviour is obviously the *use of language as the overt expression of 'thinking.'*"

Discussion: A Friendly Challenge to Professor Thorndike. (C. Spearman.)

A reply to Thorndike's paper "On the Organization of the Intellect" (*Psychological Review*, XXVIII. 141), ending with the challenge—"Let Thorndike and myself agree as to what (if any?) ascertainable fact would be in harmony with the view of one of us and not the other. Let us agree upon and carry out the same procedure of investigation. And then let us produce and explain our results side by side. If a third and impartial investigator can be found, so much the better."

Rivista di Psicologia.

Anno XVIII, Nos. 2–3.

Contemporary Psychology of Religion. (S. de Sanctis.)

A Memory Test for Normal Children. (E. Torriani and G. Corberi.)

A word-reproduction test (based on Claparède's work) is described and results are given. The ages of the children tested ranged from 7 to 13 years and it was found that maximum retention took place for boys at 9 and for girls at 12.

Notes on the Binet-Simon Scale and the 'Short Psychological Test' of Francia and Ferrari. (G. Vidoni.)

The author concludes from recent tests applied more especially to 50 pupils of the *Scuola Autonoma* for Defectives at Genoa that the concept of 'Mental Age' should be used with much caution, but can then yield practical results of some value.

The Laws of Mirror Writing and their Relation to Left-handedness. (A. Knipfer.)

This interesting article deals with the psycho-physiological conditions of mirror-writing and advances, as the explanation of the success of the first left-handed (mirror) writing on the part of a left-handed person taught to write with the right, the concept of 'co-exercise' of the left with the right. The former traces out psychically all the graphic movements which the latter actually performs, as their 'engrams' are, in the left-handed person, in the same hemisphere as the motor centres of the left hand and the higher writing centres.

Innate and Acquired Aptitudes. (O. Decroly.)

Superstition. (C. Guggenheim.)

The Educative Efficacy of Pain. (E. Pietrosi.)

The scope of this article is philosophical rather than psychological.

Applied Psychology: Against Juvenile Delinquency. (G. C. Ferrari.)

Under this heading Prof. Ferrari gives an account of work done and plann especially in Genoa, for the study and remedial education of juvenile delinquents.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETINGS.

February 24, 1923.	An Examination of Some Current Beliefs concerning Muscular Skill, by T. H. Pear.
	The Use of Mental Tests in American Schools and Clinics, by M. MacFarlane.
March 24, 1923.	The Estimate of Duration, by M. Sturt.

SECTIONAL MEETINGS.

Aesthetics Section.

January 29, 1923.	Communication *versus* Expression, by L. Abercrombie.
March 5, 1923.	Exact Terminology in Philosophy: Symmetry, etc., in Aesthetics, by Sir Charles Walston.
	The True Paradox of the Comedian, by E. Bullough.

Education Section.

January 1, 1923.	The Child and the Cinema, by C. W. Kimmins.
February 13, 1923.	The Super-sensitive Child at School: a Psychoanalytic Study, by E. Sharpe.
March 13, 1923.	The Effect of Schooling on Intelligence Tests, by H. Gordon.

Medical Section.

January 24, 1923.	Auto-suggestion and Transference, by William Brown.
February 28, 1923.	Some Observations and Criticisms of Psychotherapeutic Methods, by J. A. Hadfield.
March 22, 1923.	The Nature of Auto-suggestion, by Ernest Jones.

PRINTED IN ENGLAND BY THE SYNDICS OF THE PRESS AT THE CAMBRIDGE UNIVERSITY PRESS